Delivering Higher Pay? The Impacts of a Task-Level Pay Standard in the Gig Economy*

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Abstract

How does a task-level minimum pay requirement for gig workers affect their earnings and employment? We study this question in the context of a January 2024 law in Seattle that establishes a per-task minimum pay standard for app-based delivery workers. Drawing on novel cross-platform, trip-level gig activity data, we compare earnings and employment trajectories around the implementation of the law for workers who were doing delivery work in Seattle before the reform against workers who had been active in other regions of Washington State. We find that the minimum pay law raised delivery pay per task, though the increases in base pay per task were partially offset by a substantial reduction in average tips, a major component of delivery pay. At the same time, the policy led to a reduction in the number of tasks completed by highly attached incumbent drivers (but not an increase in exit from delivery work), completely offsetting increased pay per task and leading to zero effect on monthly earnings. We find evidence that drivers experienced more unpaid idle time and longer distances driven between tasks, but find no evidence that drivers reduced their total time working on delivery apps and only limited evidence of switching from delivery to ride-hailing work. Using a simple model of the labor market for platform delivery drivers, we show that our evidence is consistent with free entry of drivers into the delivery market driving down the task-finding rate until expected earnings return to their pre-reform level. These findings highlight the challenges of raising pay in spot markets for tasks where there is free entry of workers.

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1 Introduction

Minimum wage regulations that raise pay rates above competitive market-clearing levels often result in rationing, as the quantity of labor supplied to the market exceeds available job opportunities. Most analyses of the minimum wage focus on traditional labor markets with persistent employment relationships. In that case, rationing entails a division between employees, who benefit from higher wages (albeit perhaps with fewer paid hours), and unemployed individuals, who cannot find a covered job. However, in platform-mediated gig economy markets and other spot markets for tasks with free entry among workers, the impact of a minimum pay standard may differ substantially depending on how tasks are rationed. As a growing number of jurisdictions consider the adoption of minimum pay policies for platform-based gig work, it is important to understand who bears the benefits and costs of such regulations. In principle, gig economy pay regulations are intended to address low earnings levels among platform-based workers, who are not covered by standard minimum wage laws (Zipperer et al., 2022; New York City Department of Consumer and Worker Protection, 2022; Manzo, Petrucci and Bruno, 2022; Jacobs et al., 2024). A key question is whether such policies can effectively raise earnings for market participants in practice.

In this paper, we shed light on this question by examining the impacts of a new task-level minimum pay standard for platform-based gig work. Specifically, we study Seattle's App-Based Worker Minimum Payment (ABWMP) Ordinance, which set minimum base pay requirements for workers performing delivery tasks on gig-work apps. This law went into effect in January 2024 and applies only to deliveries starting or ending in the city of Seattle. We estimate the causal effects of the policy by comparing changes in outcomes for drivers with different degrees of pre-policy exposure to work in Seattle.

To evaluate the impacts of the pay ordinance, we draw on unique gig worker data from Gridwise Inc., which contains task-level information on workers' activities and associated revenues across major delivery and rideshare platforms. The data allow us to track individual drivers over time and provide fine geographic detail and task-level compensation broken out into base pay, tips, and other payments. Our sample covers the State of

Washington from August 2023 to July 2024, allowing us to observe gig work across all platforms before and after the law's implementation, both within and outside Seattle.

We use these data to implement a difference-in-differences design among workers who were active delivery drivers prior to the reform. Our core design compares drivers whose pre-reform delivery activity was concentrated in Seattle against drivers who drove elsewhere in Washington State before the policy was implemented. We focus primarily on workers who were highly active in the pre-policy period, as these workers are much more likely to continue to engage in delivery work afterwards. However, we also examine impacts on less-attached incumbent drivers as well. In addition, we conduct descriptive analysis to characterize post-period rates of entry into the market in each region.

First, we document that the minimum pay standard was binding and resulted in average base pay per task doubling in Seattle during a period when pay rates remained constant in the rest of the state. Crucially, however, although the policy only applies to base pay, tips are a major part of driver compensation in app-based delivery work and constitute the *majority* of total pay per task on average. We find that the Seattle pay standard led to an immediate drop in average tips per task, which may have been in part a result of changes in delivery app interfaces for Seattle-based customers. The decline in tips offsets over one-third of the increase in base pay.

Turning to the individual-level analysis, we find that while highly-attached drivers who were exposed to the reform benefited from higher total earnings per delivery task after the reform, this increase was offset by a decline in the number of tasks completed per month, beginning in the second month after the pay standard was implemented. This decline in the number of tasks per worker corresponded to a decline in the aggregate number of tasks completed in Seattle over the same period. Strikingly, combining both margins we find exposed workers experience *no* increase in total monthly earnings after the first month following policy implementation. We highlight that, despite the decline in tasks completed, exposed individuals experience an increase in total monthly *base* earnings—the component of pay targeted directly by the policy—but this is fully offset by the decline in tips. We additionally examine whether exposed workers respond to lower task demand by switching towards ride-hailing work, but find only small and statistically insignificant effects on ride-hailing tasks and earnings. When we examine less-attached workers, we find nearly identical increases in delivery earnings per task as for the more-attached workers, but find no reduction in delivery tasks completed per month. However, since the baseline number of monthly tasks completed by drivers in the less-attached sample is an order of magnitude smaller than in the more-attached sample, the net impacts on monthly earnings are small and not statistically significant.

Despite the lack of substantial long-run effects on earnings, drivers might still have been better off if their work hours declined or if they were able to maintain earnings while exerting less effort and incurring fewer costs associated with driving. To study these outcomes we use data from subsets of apps providing information on hours spent engaged with delivery apps and the locations and timing of tasks. We find no evidence that hours spent working on apps declined (though these estimates are somewhat imprecise) but we do find evidence of statistically significant and substantial declines in the number of tasks completed per hour. We also find that the minimum pay policy reduced the utilization rate (share of on-app time spent performing revenue-generating delivery tasks versus unpaid idle time) and increased wait times between tasks. Further, we find that the distance driven between subsequent tasks increased along with wait time, implying that workers continued to incur effort and vehicle costs during the additional unpaid idle time. In all, we find no evidence that incumbent drivers benefit through non-earnings margins.

Why did exposed incumbent drivers not benefit on net from the minimum pay standard? To help answer this question we interpret our empirical findings through the lens of a simple model of the app-based delivery market, closely following Hall, Horton and Knoepfle (2023) and Fisher (2024*a*). The key prediction from the model is that a trip-level minimum pay standard will only increase drivers' earnings in settings where there is not free entry of drivers and where the market-level elasticity of demand for drivers is sufficiently small. Our finding of no earnings effect suggests that one or both of these conditions is violated in the Seattle gig delivery market. Descriptive evidence documenting a growing share of activity captured by new entrants in Seattle post-reform suggests that free entry plays an important role. Without strict barriers to entry, any increase in pay per trip will be partly offset by the entry of additional drivers, leading to increased wait times to find tasks and reducing pay per hour. With free entry (i.e. an infinite labor supply) this process continues until the effective hourly wage equals that in the wider labor market and the increased pay per task is fully offset, as we find empirically.

Our paper contributes to a large empirical literature on the impacts of minimum wage policies, including more recent studies on the effects on city-level minimum wage laws (Jardim et al., 2022; Dube and Lindner, 2021; Karabarbounis, Lise and Nath, 2023).[1] Particularly relevant is the recent study by Jardim et al. (2022) examining the impacts of a city-level minimum wage in Seattle implemented in 2015 that covered traditional employment but not platform-based gig work. They find that Seattle-based workers exposed to the policy were no more likely to become unemployed than other workers in the state, but that the benefits of higher hourly wage rates were partially—but not fully—offset by lower hours for continuing workers. In contrast to our setting where tasks are distributed among all market participants, they find that the higher minimum wage reduced entry of new workers, who faced increased difficulty finding traditional jobs.

We also add to a growing literature documenting the importance of responses to the minimum wage on margins beyond employment and wages (Clemens, 2021; Liu et al., 2024; Davies, Park and Stansbury, 2024). In particular, we examine a setting in which tips are a major component of overall earnings, but were not directly covered by the minimum pay standard. We find that documenting adjustments on the tip margin is crucial to capturing the full effects of the policy on the intended beneficiaries.

Our work is most directly related to recent research studying the impacts of pay policies and other regulations in the online platform-based gig economy. Koustas, Parrott and Reich (2020) study how the implementation of a minimum pay standard for ride-hailing app drivers in New York City in 2019 impacted demand for trips, finding a route-level price elasticity of -0.68. Horton (2025) studies an online labor market that

[1] For earlier evidence and comprehensive reviews, see Card and Krueger (1995); Brown (1999); Neumark and Wascher (2010); Dube and Lindner (2024).

randomly assigned minimum hourly wage rules to a subset of job postings, and finds that "employers" in this market became more selective about the types of workers they hired, and that reductions in hours worked per task largely offset the increases in hourly wage. Most directly related to our findings is a study by Hall, Horton and Knoepfle (2023) which documents that increases in pay rates per trip implemented by Uber led to an increase in entry and longer wait times for trips (lower utilization rates) that exactly offset, such that hourly earnings did not change for drivers in affected markets.

The paper proceeds as follows. Section 2 provides background on working on online delivery platforms and the Seattle minimum pay ordinance for app-based delivery workers. Section 3 describes the Gridwise data and details our research design. We present our descriptive results of aggregate market trends in Section 4, and our individual results in Sections 5 and 6. Section 7 interprets our findings through the lens of the model and discusses policy implications.

2 Institutional Background

This section briefly summarizes the key features of online delivery platforms and the Seattle minimum pay ordinance for app-based delivery workers.

2.1 Working on Online Delivery Platforms

On-demand delivery services facilitated through online platforms have experienced fast worldwide growth over the past decade, particularly since the onset of the COVID-19 pandemic.[2] This new type of service has generated substantial work opportunities for independent couriers who can self-schedule their delivery work, and has become one of

[2]For example, Statista reports that the global online food and grocery delivery market has generated \$1.21 trillion in revenue in 2024, with rapid average annual growth of up to roughly 50% during the COVID-19 pandemic in 2020 and 2021. They forecast that this market will achieve a compound annual growth rate of 9.33% from 2024 to 2029, expanding to a projected \$1.89 trillion globally by 2029. See https://www.statista.com/outlook/emo/online-food-delivery/worldwide (accessed January 10, 2025). Garin et al. (2025) document a substantial rise in platform-mediated delivery (and transportation) work from 2012 through 2023 in the United States, with a dramatic increase in delivery work and a significant shift from ride-hailing to delivery work around the COVID pandemic.

the most important services in the gig economy.[3]

In the online delivery market, platforms dispatch delivery tasks generated from customer orders to workers who pick up and deliver products from merchants to customers. On most platforms, workers begin by opening the driver-facing app and indicating that they are available to complete delivery tasks, at which point the app begins searching for available nearby tasks. After a wait, workers are offered individual delivery tasks, which they can either accept or reject (either proactively or by letting the offer time out). Offers generally contain information about the pickup location (typically close to the driver's current location), drop off location, estimated time and distance, and total pay. On most apps, the guaranteed pay rate for the trip is *inclusive* of tips, which are commonly made by customers upon placement of their orders.[4] Drivers can turn down tasks, but doing so typically leads to them being offered fewer and less desirable tasks in the future. At the conclusion of a delivery task, workers must decide whether to remain at their previous drop-off destination or to drive towards a desired pick-up spot. Because tasks are assigned to drivers near pick-up locations, and since drop-off destinations are often in residential areas, drivers are encouraged to drive towards "hot" zones with more restaurant activity in order to receive more frequent and more desirable offers—though they must do so at their own expense.[5]

In contrast to ride-hailing work, where tips are a small part of total earnings, the *majority* of delivery compensation typically comes from tips. Appendix Figure B.3 shows the pay structure on major delivery and rideshare platforms in Washington State between August 2023 and December 2023.[6] Tips account for between 52 and 62 percent of base pay and tips per task on major delivery platforms, while they only make up about 12 percent on rideshare platforms, suggesting that delivery workers generally rely more on tips for a

[3] Garin et al. (2025) document that platform-mediated delivery and transportation work has been the largest component of gig work since 2017, and present evidence that most of the expansion of platform work since 2020 has been driven by delivery work.

[4] When they complete a task or when they no longer plan to work and end a driving session, they can observe total pay from the completed tasks, broken out into base pay and tips. Appendix Figure B.1 provides a typical example of what information drivers see before and after engaging in a delivery task.

[5] Drivers may also opt to drive towards their initial pick-up location to avoid drifting too far from their home location.

[6] Authors' analysis of Gridwise data.

significant portion of their income than rideshare workers do.[7] This is a consequence of the tendency of delivery customers to make tips that scale with the value of the meal, rather than with the delivery fee. Given the generous tip amounts, apps can offer attractive jobs to workers while providing relatively low base pay.

2.2 Seattle Minimum Pay Ordinance for App-Based Delivery Workers

On May 31, 2022, the City of Seattle passed a minimum pay ordinance for app-based delivery workers, known as the App-Based Worker Minimum Payment (ABWMP) Ordinance (hereafter, the ordinance). This ordinance followed previous city-level initiatives that implemented substantial increases to the minimum wage and created minimum pay standards for ride-hailing work (but did not apply to platform-based delivery work).[8] The delivery pay ordinance mandated that the minimum base compensation for delivery tasks resulting in engaged time or engaged miles exceed the greater of i) \$0.44 per minute plus \$0.74 per mile, or ii) \$5 per offer, for each offered task.[9,10] The ordinance came into effect on January 13, 2024.

The ordinance covers app-based delivery services performed in Seattle.[11] If the engaged time of a service begins in Seattle, the requirements of the ordinance apply, regardless of where the service terminates. If the engaged time begins outside of Seattle,

[7] Jacobs et al. (2024), analyzing Gridwise data over a two-week period in January 2022 in Los Angeles and San Francisco Bay metros and in Boston, Chicago, and Seattle metros document qualitatively similar findings. Gridwise's 2025 Annual Gig Mobility Report also supports these findings (Gridwise Analytics, 2025).

[8] See Jardim et al. (2022) for a discussion of the city-level minimum wage reform.

[9] "Engaged time" begins upon the app-based worker's acceptance of the offer and ends upon the app-based worker's completing performance of the offer, cancellation of the offer by a customer or the network company, or cancellation with cause of the app-based worker's acceptance of the offer. "Engaged miles" refer to miles traveled during engaged time. If an app-based worker accepts a new offer during performance of a previously accepted offer, and both offers are facilitated or presented by the same network company, engaged time and engaged miles accrued during any period of time in which performance of the offers overlaps shall be subject to the minimum compensation requirements for a single offer. Tips and incentives paid to an app-based worker do not count towards the minimum payment. For details, refer to https://library.municode.com/wa/seattle/codes/municipal_code?nodeId=TIT8LAST_CH8.37ASEWOMIPA (accessed August 6, 2024).

[10] See https://www.seattle.gov/laborstandards/ordinances/app-based-worker-ordinances/app-based-worker-minimum-payment-ordinance (accessed August 6, 2024).

[11] The ordinance covers services facilitated by network companies that mediate work performed by 250 or more app-based workers worldwide.

the ordinance applies only for the portion of the service that occurs within Seattle. In other parts of Washington State outside Seattle, there are no minimum pay regulations for app-based delivery workers. More generally, during the period we study, there were no other major changes to city-level labor market regulation. The Seattle ride-hailing pay standard had initially been implemented on January 1, 2021, and was then superseded by a statewide regulation (State House Bill 2076), which took effect on January 1, 2023. Hence, the ride-hailing pay standard applied equally in all parts of Washington State throughout the period we study.[12] This policy variation motivates our research design below.

Following the ordinance's implementation, many delivery platforms responded by making changes to their apps to make costs induced by the ordinance salient to consumers and delivery workers. For example, DoorDash, Instacart, and Uber Eats imposed a flat \$4.99 or \$5 fee on Seattle orders. As shown in Figure 1, Seattle consumers now have to pay a new \$4.99 regulatory response fee on DoorDash highlighted at checkout, while consumers in Spokane do not. Some platforms went further and no longer allowed customers in Seattle to tip at checkout. For example, on Uber Eats, Seattle consumers cannot provide a tip before delivery is complete, as shown in Figure 2, and can only add it after delivery.[13] By contrast, there are no such tip policy changes in Spokane.

3 Data and Research Design

An empirical analysis of minimum pay standards in the gig economy requires task-level gig activity and earnings data. As self-employed independent contractors, gig workers do not pay into state unemployment insurance (UI) systems, which means that gig workers' earnings do not appear in quarterly UI earnings records. Administrative tax return data provide broad coverage of gig-worker earnings, but do not provide the geographic or other detail required to study the Seattle minimum pay ordinance we focus on. Proprietary

[12]For details on the Washington State House Bill 2076, refer to https://lawfilesext.leg.wa.gov/biennium/2021-22/Pdf/Bills/Session%20Laws/House/2076-S.SL.pdf (accessed August 6, 2024).

[13]See https://www.uber.com/blog/uber-delivery-tip-policy-seattle/ (accessed August 6, 2024).

internal data from gig economy platforms, while fine-grained, only provide insights into labor supply of individuals on a particular gig economy platform, and do not speak to behaviors of these individuals on other platforms. To overcome these data limitations and facilitate research on minimum pay standards in the gig economy, we use unique data from Gridwise Inc. that tracks detailed worker activity and earnings across multiple gig platforms. This section first introduces the Gridwise data source and then details our research design.

3.1 Gridwise Data

Our main data set consists of task-level gig platform information collected by the Gridwise app. Gridwise is a third-party gig work assistance app that allows gig workers to link all of the various delivery and rideshare platforms they use and sync their gig driving activity in order to help them, for example, track earnings, mileage, and expenses, optimize activity across platforms, and prepare tax returns.[14] Through their app, Gridwise automatically collects users' real-time gig driving activity and earnings data. To date, Gridwise has collected data on over 720 million trips and $8.3 billion in driver earnings. A unique aspect of Gridwise data is that it provides visibility to gig workers' activity and earnings across platforms, which presents comprehensive insights into the labor supply of gig workers.

Gridwise is the only available source we are aware of that contains consistent information on all the delivery and rideshare trips done by individuals over time and across apps.[15] Still, a limitation of the data is that it only covers the minority of drivers who are enrolled on the Gridwise app. Gridwise reports that quarterly trends in their data tightly follow the aggregates reported by the apps, with up to 98% correlation with key quarterly figures reported by major gig platforms (Gridwise Analytics, 2025). While there is no authoritative data on the number of drivers active on all apps, we compare

[14]Gridwise offers a free version and a premium service, Gridwise Plus, with prices starting at $9.99 per month or $6 per month billed annually. See `https://gridwise.io/plus/` (accessed January 10, 2025). Note that data are collected for all users, irrespective of version.

[15]Appendix Figure B.15 shows that, in our data, approximately 25% of drivers use multiple delivery apps in a given month, and approximately 15% use multiple apps on a given day.

the Gridwise sample sizes against the 2023 Census Nonemployer Statistics (the most recent available year), which tabulates the number of self-employed taxpayers by state and industry.[16] This comparison suggests that about 10% of platform drivers are covered by the Gridwise app. Since installing the app requires some effort and foresight, workers who do more gig work are likely to be over-represented relative to those who do gig work only intermittently. We note, however, that these more-intensive drivers are a primary focus of our individual analysis because they are most likely to be impacted by the policy and are the drivers the policy reforms aim to benefit. We do not expect there to be any difference in coverage between treatment and control regions, nor do we expect the policy to impact coverage rates in either the treatment or control region.[17]

We use data on all tasks performed by Gridwise users in Washington State between August 2023 and July 2024, giving us roughly six months of data before and after the Seattle minimum-pay ordinance went into effect. The data cover major delivery and rideshare platforms including DoorDash, Grubhub, Instacart, Uber Eats, Lyft, and Uber. The structure of the data collected by Gridwise varies by gig platform. For Grubhub, the core reporting unit of work is a single task involving one pickup and its associated drop-off. For Instacart, Uber Eats, Lyft, and Uber, the work unit is either a single task or a batch with multiple tasks offered as a single job to workers, while for DoorDash, the unit is a shift, typically a driving session, which can consist of multiple tasks and/or batches. For each unit of work ("activity" hereafter), we observe the unique worker ID and platform ID, worker earnings including base pay, tips, bonuses, and total earnings, request, start, and end times, start and end locations (census block level), and number of tasks.[18] In addition, the Gridwise data also include a *task-level* breakdown for platforms where the unit of work is a shift or a batch, which allows us to observe the components

[16]The 2023 Census Nonemployer Statistics for Washington State report 38,993 self-employed couriers and 22,800 self-employed drivers with income tax filings (U.S. Census Bureau, 2025). Note that this figure likely undercounts gig workers who do not file a Schedule C, but it also includes all couriers and taxi/limousine drivers, possibly leading to an overcount.

[17]Communication with the Gridwise team indicates that the team never sent any specific notifications or launched any campaigns regarding the policy towards their app users.

[18]Request time refers to the time when the offer was accepted by the worker (not available for DoorDash). Start and end times/locations refer to the times/locations of pickup and drop-off (for DoorDash, the start and end of the driving session).

of pay for each individual delivery order but not the precise request, start, and end times or locations of the delivery within the broader reporting unit.[19] The data include 2,844,465 tasks completed by 5,930 workers, generating a total of $35,366,044 in worker earnings. Among these, 4,492 delivery workers performed 1,939,592 tasks, earning a total of $18,899,728.[20]

We use the unique worker IDs to aggregate across tasks to create a worker-level dataset containing monthly delivery and rideshare tasks and earnings (including base pay, tips, bonuses, and total earnings) and exposure to work in Seattle based on the driver's share of pre-ordinance delivery earnings from Seattle. We define months as calendar-month periods relative to January 13, 2024.[21] Gridwise separately provides data on payments made by platforms to workers that are not associated with a single task, which are either incentive payments for achieving a target or adjustment payments to ensure after-the-fact compliance with regulations or promises made to the worker.[22] We allocate these payments to a given worker-month, though we cannot attribute them to specific trips.

The Gridwise data contains information about when and where delivery tasks took place, but the information is inconsistently reported across apps. Ideally we would observe both total on-app time and the request and end time/location of each delivery task, which would enable us to directly measure revenue-generating time separately from unpaid time cruising or idle while waiting for tasks. In practice, though, we observe total on-app time (the best available measurement of total hours worked) for DoorDash (but not the request and end time/locations of specific trips), while for Uber Eats and Grubhub, we observe the request and end time of individual trips but not overall on-app time. In order to create measure of total hours worked across apps, we develop a procedure to use the available

[19]The start and end locations of individual tasks are at the much less granular level of Core Based Statistical Areas (CBSAs), which does not allow us to distinguish whether individual tasks start or end within the city of Seattle.

[20]These totals represent earnings allocated to specific trips before adding in the supplemental payments discussed next.

[21]Months in our analysis go from the 13th of each calendar month through the 12th of the following calendar month.

[22]For instance, if a task takes longer than expected (for instance, due to restaurant delays) and the guaranteed pay for a delivery task that was calculated based on an initial estimate of task distance and duration turns out to be less than required by the minimum pay standard, the app may make a separate adjustment remittance to the worker.

information to impute total on-app time across all apps. In addition, we construct more detailed measures of time use that are only defined for activities on certain apps. In particular, for Uber Eats and Grubhub trips, we use the request and drop-off time data and start and end location data to measure average time and distance between subsequent delivery tasks and to calculate the utilization rate as the share of total monthly hours worked that were revenue-generating trip time, following Jacobs et al. (2024).[23]

3.2 Empirical Design

The primary goal of this paper is to estimate the causal impact of the minimum pay policy on individual workers. To that end, we follow workers who were active in delivery work during the pre-implementation period and measure differences in post-implementation outcomes for workers who were more vs. less exposed to the policy. As described in Section 2.2, Seattle's minimum pay ordinance applies to tasks rather than workers. The ordinance (fully or partially) covers tasks that start or end within the city's boundaries. We measure a worker's exposure to the policy based on the share of their pre-policy activity that occurred in Seattle.

Specifically, we define exposure as follows. First, because not all tasks in our data can be geocoded individually at the city level, we classify activities as i) definitely starting or ending in Seattle (denoted as S); ii) definitely not in Seattle (starting and ending in Washington State outside King County (which contains Seattle); denoted as N); or iii) in buffer zone (starting (ending) in other parts of King County outside Seattle and ending (starting) not in Seattle; denoted as R).[24] Second, for each worker, we define exposure as $S/(S + N)$, the share of *classifiable* pre-policy delivery earnings coming from Seattle activities.[25] We adopt this conservative approach to defining exposure to be

[23] Details on the imputation method are provided in Appendix Section A.4. Appendix Table C.5 shows evidence that the results are robust to alternative imputation assumptions.

[24] We have census-block-level geographic information for tasks on gig platforms where the unit of work (an activity) is a single task, which allows us to identify whether a task starts (ends) within or outside Seattle's boundaries. For platforms where an activity represents a shift or a batch, we don't have such granular geographic information available for encompassed tasks. See discussion in footnote 19.

[25] Workers who *only* perform delivery work classified as in the buffer zone—primarily in King County outside Seattle—in the pre-policy period are excluded from the analysis in order to mitigate the risk of these workers switching to the Seattle market post-policy and generating potential policy spillovers.

confident that individuals with zero measured exposure did not drive in Seattle in the pre-implementation period. Since we exclude tasks that start and end in King County outside Seattle (R tasks) from this exposure measure, workers with 100% measured exposure may still do some of their pre-reform trips outside Seattle in this buffer region. Appendix Table C.1 shows that, in practice, drivers with 100% exposure under this definition performed only half of their pre-reform tasks in Seattle, while the other half are done in the buffer zone.

For simplicity, we use a discretized version of the exposure measure in our main analysis, in which we code a worker as exposed ("treated") if their exposure is greater than 80% and not-exposed ("control") if the exposure is less than 20%. In practice, this simplification is not restrictive, as over 95 percent of workers in our sample are distributed at extremes beyond these two thresholds, as shown in Appendix Figure B.6. In robustness tests, we show that our main results are qualitatively unchanged when we use the continuous exposure measure (see Appendix Tables C.6, C.7, C.8, C.9, and C.10).

Throughout our analysis, we present results separately for workers with higher or lower degrees of attachment to the gig delivery market in the pre-policy period. We define more-attached workers as those who performed delivery tasks above the median in the pre-policy period (about 20 tasks per month). Our main analyses focus on this group, as attached workers are significantly more likely to continue to do delivery work in the post-reform period than incumbent drivers with low levels of attachment, and are therefore most likely to be impacted by the policy. However, we also present impacts on incumbent drivers with lower attachment.

Table 1 presents means and standard deviations of key worker characteristics prior to policy implementation for the exposed (Seattle) and not-exposed (non-Seattle) worker samples (columns 1–4), and estimates of differences in characteristics between the two groups (columns 5-6). Panel A shows the statistics for the subsample of more-attached workers. In this subsample, Seattle's workers performed approximately 106 delivery tasks per month and worked for 4 out of 5 months, on average, before policy implementation, which implies an average of about 133 delivery tasks per *active* month during the pre-policy period. Within this same subsample of attached drivers, non-Seattle workers have similar earnings per task and days active per month as those in Seattle, but complete slightly fewer delivery tasks per month. Panel B presents summary statistics for the subsample of less-attached workers. Notably, the average earnings, tasks completed, and days active among this subsample in the pre-policy period are an order of magnitude smaller than the corresponding amounts for the more-attached workers.

To estimate the causal effects of the minimum-pay policy, we use a dynamic difference-in-differences design comparing the differential evolution of outcomes for exposed ("treated") and not-exposed ("control") workers before vs. after policy implementation. Specifically, we estimate the following specification:

$$Y_{it} = \sum_{k \neq -1} \beta_k \text{Treat}_i \times \mathbb{1}\{t = k\} + \alpha_i + \zeta_t + \epsilon_{it}, \tag{1}$$

where Y_{it} is one of several labor market outcomes for individual worker i in event month t, measured relative to January 13, 2024, the date when the Seattle minimum pay ordinance went into effect. In our main analysis, we examine ten sets of individual-level outcomes on delivery work: average pay per task, any work, number of completed tasks, earnings, hours worked, active days, average number of tasks completed per hour, utilization rate, average task wait time, and average distance between tasks. We additionally examine three outcomes for rideshare work in the appendix, including any work, number of completed tasks, and total earnings. Individuals who do not appear in the Gridwise data for a given event month are coded as not working, completing zero tasks, earning zero dollars, working zero hours, and working zero days in that event month, and are excluded from the analyses of average pay per task, average number of tasks completed per hour, utilization rate, average task wait time, and average distance between tasks for that event month.

The term Treat_i is an indicator for whether worker i was exposed to work in Seattle prior to policy implementation. α_i indicates individual worker fixed effects and controls for worker characteristics that are constant over time. ζ_t indicates event year-month fixed effects and controls for factors that are constant across workers but vary over time.

Standard errors are clustered at the worker level. The coefficient of interest is β_k. $k = 0$ corresponds to the first event month after January 13, 2024, when the ordinance became effective. The event month before the effective date, $k = -1$, is omitted from the estimation in order for the model to be identified. Each β_k measures the difference between exposed ("treated") and not-exposed ("control") workers in a given event month relative to the difference in the event month prior to policy implementation. To the extent that identification assumptions hold (discussed below), the β_ks identify the average treatment effect on the treated (ATT) of the minimum pay ordinance on individual worker outcomes, in month k relative to ordinance implementation. In addition to the fully dynamic difference-in-differences specification, we also estimate a "static" specification by replacing the indicators for event months interacted with the term Treat_i in Equation (1) with a single indicator for the post-ordinance period to obtain the average effect of the ordinance. Results appear in Sections 5 and 6.

The key identifying assumption of our difference-in-differences design is that outcomes for exposed ("treated") and not-exposed ("control") workers would have evolved in parallel in the absence of policy change. As a check, we test for differences in pre-reform trends across groups by examining estimates of β_k over the pre-policy period ($k < -1$) after estimating Equation (1). As shown in Section 5, we find no evidence of pretrend violations. In robustness tests, we also estimate a modified version of our main specification, Equation (1), including a fixed set of pre-policy worker covariates—delivery (rideshare) tasks, months active in delivery (rideshare) work, share of earnings from rideshare versus delivery work, and log total earnings, during the pre-policy period—interacted with indicators for event months to control for potentially time-varying impacts of baseline worker characteristics. As shown in Section 5, the results are robust to adding these additional controls. These tests support the identifying assumptions required for the validity of the difference-in-differences research design.

4 Descriptive Analysis of Aggregate Trends

Before documenting the minimum pay policy's impacts on individual workers, we first present descriptive analysis of how the delivery market evolved in Seattle around the policy's implementation. To this end, we aggregate delivery tasks by app and market, assigning all tasks in the data (not just those done by incumbents) to Seattle or the rest of Washington outside King County based on the start and end location of their encompassing activity as described in Section 3.2.[26] We focus on two sets of aggregate outcomes on delivery work: average pay per task and the total number of completed tasks.

First, we find that base pay per task more than doubles in Seattle after the enactment of the policy. Figure 3 plots the trends in average base pay, tips, and total pay per task on the four largest delivery platforms, DoorDash, Grubhub, Instacart, and Uber Eats, in each event month relative to ordinance implementation, separately for tasks that are part of an activity that passes through Seattle and those that start and end in Washington State outside King County. Month 0 is the first event month post-ordinance, and month -1 is the event month before the ordinance. In each panel, outcomes in both regions are relatively stable throughout the pre-policy period. Following the implementation, there is an immediate increase in base pay per task in Seattle, as shown in Panel A. The effect persists over time and is qualitatively similar across platforms.[27] By comparison, no meaningful changes happened in the remainder of Washington outside King County. These results provide suggestive evidence that the minimum pay standard is binding, that post-implementation pay rates are well above the five dollar lower bound, and that platforms are complying with the ordinance following its implementation. The lack of any effect outside of Seattle indicates that there are no discernible spillover effects of the ordinance to areas outside of King County.

To provide further detail about the shift in base pay rates after the minimum pay

[26] We exclude activities classified as in the buffer zone, those primarily in the King County market outside Seattle, from the comparison group and analysis in order to avoid potential policy spillovers.

[27] Average base pay per task on Instacart before the ordinance and its rise post-ordinance are both a bit larger than on other platforms because of the additional in-store shopping time required prior to delivery.

reform, Figure 4 plots the distribution of base pay amounts per delivery in Seattle during the pre-reform and post-reform periods, respectively. The figure confirms that while a majority of trips had base pay less than five dollars before the pay standard went into place, essentially no trips had base pay levels below the five-dollar minimum afterwards. Notably, however, there is very little bunching of base pay at the five-dollar minimum in the post-period; instead, most of the base pay distribution shifts to higher dollar amounts. This finding suggests that the formulary minimum pay rates based on time and distance generally bind, rather than the five-dollar absolute minimum.

At the same time, however, Panel B of Figure 3 shows immediate declines in average tips per task in Seattle. While we observe a decline in tips per task across all platforms, the magnitude of the decline varies significantly across apps. In particular, the decline in tips per task is notably larger on Instacart and Uber Eats, the platforms that disabled up-front tipping in Seattle in response to the ordinance, as described in Section 2.2. As a result of the decline in tipping, the increases in *total* earnings per task are smaller than the increases in base pay, as shown in Panel C, with total earnings per task rising less on Uber Eats and Instacart than on DoorDash and Grubhub. In Appendix Figure B.4, we likewise find that the shift in the distribution of *total* payment amounts per trip is more muted than the shift in base pay rates shown in Figure 4.

We next examine how the overall volume of tasks observable in the Gridwise data evolves around policy implementation. Figure 5 plots total monthly deliveries in Seattle and the rest of Washington State (excluding King County), normalizing all amounts by each region's respective pre-ordinance average monthly total tasks. The top lines in Figure 5 show that while the total number of tasks in each region trended similarly before the pay standard went into effect, the volume of tasks completed in Seattle persistently declined after its implementation, both in absolute terms and compared to trends in the rest of the state. The largest decline occurs in the second month after the policy implementation. These aggregate trends are consistent with higher costs per delivery resulting in reduced total demand for delivery tasks in Seattle compared to the remainder of Washington. An important caveat is the reported numbers reflect only tasks recorded among Gridwise users rather than all delivery tasks performed on each platform.

However, strikingly, Figure 5 shows that the decline in delivery tasks in Seattle is driven entirely by a reduction in tasks completed by incumbent drivers who were active prior to the reform. While tasks completed by incumbents fall sharply in Seattle after the ordinance went into effect, we find that the volume of deliveries performed by new entrants evolves nearly identically in Seattle as in other regions of Washington. Within three months of the reform, post-reform entrants account for a majority of all tasks in Seattle, while incumbents complete twice as many tasks as entrants elsewhere in the state. On the whole, these results suggest that even as the delivery market in Seattle contracted post-ordinance, there was a relative influx of new workers responding to increased pay per task who competed with incumbent drivers for a shrinking pie.

5 Effects on Individual Earnings

In this section, we examine the impacts of the ordinance on individual workers active in delivery work prior to implementation. We first discuss impacts on the high-attachment sample and subsequently examine effects on less-attached workers. Figures 6, 7, and 8 plot estimates of β_k for the main outcomes from estimating Equation (1) for more-attached workers, and Appendix Figure B.12 plots estimates for less-attached workers. Tables 2, 3, and 4 summarize estimates of the respective average effects. We report both baseline results without including controls and results including the full set of controls, but will focus on discussing baseline results throughout the section, since the controls have minimal quantitative impact.

We first examine impacts on average earnings per task for highly-attached workers in Figure 6. In the legend of the figure, we show highly attached exposed workers earned an average of \$4.87 base pay per delivery prior to ordinance implementation. We find that after implementation, these drivers saw an immediate, persistent increase in average base pay per delivery. Pooling all post-period months, base earnings per trip increased by \$4.09 on average as reported in column (1) of Table 2 Panel A. While this reflects a substantial increase over the pre-period average base pay rates, the magnitude of the increase is notably smaller than the descriptive increases in average base pay per task observed in Figure 3 Panel A. This difference arises due to our focus on exposed *individuals* in Figure 6 as opposed to exposed *deliveries* in Figure 3. As discussed in Section 3.2, our exposure measure is constructed such that individuals with 100 percent exposure still do many trips in the "buffer region" of King County outside Seattle—Appendix Table C.1 reports that, in practice, 100% exposed drivers only do about half of their deliveries in Seattle in the pre-period. Thus, one should expect the worker-level changes in base pay per trip using this exposure definition to only be about half the magnitude of the increase observed for Seattle deliveries, since half of exposed workers' deliveries are not covered by the new pay standard.[28]

However, Figure 6 shows that the increases in average base pay per delivery were offset by declining tips following the ordinance. Column (3) of Table 2 Panel A reports that their average tips declined by \$1.51 from a pre-ordinance average of \$5.12 per task. This decline offsets over one-third of the increase in base pay per task reported in column (1). At the same time, we find that the effects on base pay are potentially understated, as the minimum pay ordinance increases supplemental payments drivers receive that are not tied to any specific trip. Averaging these payments across trips, column (5) reports that these payments increase by \$0.91 per trip after the reform.[29] Overall, these effects result in a net increase in total pay per task of \$3.56 from a pre-ordinance average of \$10.37, as shown in column (7) of Table 2 Panel A and Figure 6.

We next examine how the reform impacted the number of deliveries completed by exposed drivers. To assess extensive-margin effects on continued participation in delivery work, Figure 7 Panel A examines an indicator for performing any delivery work. We find zero effect of the ordinance for attached workers. Thus, the ordinance did not lead to

[28]In addition, the results in Appendix Table C.1 show that Seattle-based incumbent drivers further shift their activity towards the buffer zone after the minimum pay reform. Importantly, we find no evidence that control drivers shift towards the Seattle market in response to the minimum pay standard.

[29]The results in Appendix Figure B.8 Panels C and D show that these effects reflect an increase in *ex-post* pay adjustments, rather than changes in incentive or bonus pay. Such payments are made to ensure workers' pay complies with the minimum pay standard in cases where trips take longer than expected and the up-front offers fall short of required pay minimums.

any significant exit from delivery work among highly attached workers (see also column (1) of Table 3 Panel A).

However, while the probability of completing *any* tasks was unaffected by the reform, we find that it reduced the *number* of tasks completed by continuing workers. Figure 7 Panel C evaluates the number of delivery tasks completed per month in logs (defined only for months in which drivers completed at least one task). While we observe no decrease in trips completed in the first month after implementation (month 0), we find that continuing attached workers completed about 20%-30% fewer monthly tasks in each month afterwards. This dynamic pattern is consistent with the trends in Figure 5, which showed the biggest decline in Seattle tasks occuring in the second month after implementation. This lagged response contrasts with the immediate positive effect on pay per task after the ordinance went into effect, suggesting a gradual demand response and/or a delay in the entry of new drivers. In the pooled specification, we estimate an average reduction of 26 percent in monthly tasks over the entire post-ordinance period (column (5) of Table 3 Panel A). Combining the extensive and intensive margins, Figure 7 Panel B displays effects on deliveries per month in levels, inclusive of zeros. The results are largely consistent with the effects in logs—we find a decline of 10-20 tasks per month from a pre-period average of 106.[30]

Together, Figures 6 and 7 suggest that, for highly attached workers, the ordinance increased pay per delivery, but decreased the number of delivery tasks completed per month after first post-policy month. Strikingly, Figure 8 shows that when we combine these opposing effects and evaluate monthly total delivery earnings, there is *no* effect on total delivery earnings among highly attached workers after the first month.[31] In the

[30] Appendix Figure B.7, which plots mean outcomes for the treatment and control groups over time, shows that there is attrition from the sample as workers stop driving in the post-period, but that attrition rates are identical across the treatment and control groups (consistent with no extensive margin effect). This attrition can explain why the effects sizes on outcomes in levels (inclusive of zeros) attenuate over time, even though the effects on logged outcomes (conditional on positive values) do not attenuate. If the intensive margin effect on log trips (conditional on driving) remains constant over time, but an increasing portion of both the treatment and control groups stop driving over time (and therefore have zero trips irrespective of treatment status), the unconditional effect on trips including zeros should diminish over time.

[31] In Figure 8, as in 6, the total earnings outcome incorporates bonuses, incentives, and other pay adjustments not attributed to a single trip in addition to base pay and tips; thus, the effect on total pay is not an exact sum of the respective effects on base pay and tips.

first month (month zero), monthly earnings rise as earnings per trip jump immediately before any significant decline in tasks occurs. However, after the initial month, the opposing effects of increased pay per task and reduced number of tasks offset, leading to no net impact on monthly delivery earnings. Pooling the entire post-ordinance period we estimate a null average effect on monthly delivery earnings among highly attached workers (see column (7) of Table 4 Panel A).

In Figure 8, we additionally break down the effect on monthly total earnings into the contributions from base earnings and from tips. Interestingly, we find that even with declining deliveries per month, the ordinance led to an increase in monthly total base pay for exposed workers, as intended. While the magnitude of the increase attenuates after the initial month, declining trips do not fully offset the increases in base pay per trip. Thus, if average tips per delivery had remained unchanged, the policy would have increased drivers' monthly earnings on net. However, since tips comprise the majority of delivery pay and *did* fall after the reform, the decline in monthly tip earnings exactly offsets the base earnings gains and drives the change in total earnings down to zero (see also columns (1) and (3) of Table 4 Panel A).[32]

Up to now, we have seen no meaningful increases in delivery earnings among attached workers. In theory, workers can offset lower delivery tasks by engaging more in other gig work of similar nature, for example, rideshare work. Appendix Figure B.11 Panels A, B, and C examine, respectively, an indicator for any rideshare work, the number of rideshare tasks completed, and rideshare earnings. While we do find some evidence of a small increase in rideshare work, the associated rise in earnings is negligible and not statistically significant. On net, total earnings among attached workers remain approximately unchanged when incorporating the rideshare margin.

[32]We showed earlier that the decline in tips appears to have been in part driven by the decision of Uber Eats and Instacart to disable up-front tipping at checkout. Hence, it is possible that the gains in base pay would not have been fully offset by declining tips had up-front tipping remained an option on all apps. What is not fully clear is whether the decision to disable up-front tipping was part of the optimal profit-maximizing response by platforms to the minimum pay ordinance (to reduce customer attrition, for example) or a strategy that was meant to increase political opposition to the reform among drivers but did not increase platform profits in the short run. If the latter were the case, then the steady-state impact on drivers' tips (once the ordinance were accepted as permanent by all parties) would be less negative than what we estimate here.

We next turn to the results for the sample of less-attached incumbent workers, who did 20 or fewer monthly tasks over the five months leading up to the policy implementation. Panel B of Tables 2, 3, and 4 and Appendix Figure B.12 present the main results for this group of less-attached workers. In Table 2 Panel B and Appendix Figure B.12 Panel A, we find a nearly identical effects on per-task base pay, tips, and total earnings as for more-attached workers. Meanwhile, in contrast to the more-attached drivers, we find no significant declines in the number of delivery tasks completed per month in columns (3)-(6) of Table 3 Panel B and Appendix Figure B.12 Panels B and C—though it should be stressed that only a small minority of less-attached drivers remained active in the post-policy period at all, so the effects on logged outcomes conditional on activity should be interpreted with extreme caution.

Overall, we find in column (7) of Table 4 Panel B and Appendix Figure B.12 Panel D that the dollar increase in total monthly delivery earnings for less-attached workers (inclusive of zeros) is small and positive although not statistically significant. One should note, however, that while these effect magnitudes are small, they represent a roughly 30 percent increase over baseline monthly earnings. Thus it is possible that although the effects are too small for us to detect in our sample, small earnings gains accrued across a large number of occasional delivery drivers, potentially including post-reform entrants and infrequent drivers who are less likely to sign up for the Gridwise app.[33]

Overall, this section suggests that, among attached workers, the ordinance led to higher total delivery earnings in the first event month and then no net increases in delivery earnings afterwards. This pattern is driven by the following two facts. First, higher delivery base pay per task was offset by an immediate fall in tips per delivery task. Second, workers completed fewer delivery tasks per month after the initial event month. Less-attached workers experienced a similar increase in delivery pay per task, but no decreases in delivery tasks completed per month. They achieved some gains in delivery earnings, but these gains are small and not statistically significant.

[33] Fisher (2024*b*) argues that small welfare gains distributed across a very large set of occasional gig economy drivers may amount to substantial welfare improvements in the aggregate.

6 Effects on Individual Time Use and Implications for Workers

While the minimum pay law had no substantial long-run effects on driver earnings, it may still have improved driver well-being if drivers were able to maintain their earnings levels while reducing their hours spent on the app, enjoying more down time, or reducing their expenses associated with delivery driving. Importantly, though we observe a reduction in tasks completed per month, this does not necessarily indicate a reduction in total time spent working—if tasks become scarce, utilization rates (the share of working time spent on revenue-generating tasks) may fall as drivers spend more time driving in search of delivery offers (Jacobs et al., 2024). While we do not observe comprehensive data on driver time use or expenses, this section examines the available evidence.

We first study whether the minimum pay standard led to a reduction in the total hours that drivers spent doing delivery work. In theory, it is possible that higher pay rates would lead to a reduction in hours worked, due either to strong income effects or income-targeting behavior.[34] The empirical challenge, however, is that we do not consistently observe the total time that drivers spend engaged in delivery work. Ideally we would observe both revenue-generating hours and unpaid hours spent waiting or driving in search of new tasks. In practice, though, we observe *total* on-app time (the best available measurement of total hours worked) only for DoorDash sessions; for other apps, we only observe time on revenue generating trips. Accordingly, we examine effects on hours using two approaches: using a measure of hours worked constructed by imputing missing information and using only the hours worked on DoorDash, with both approaches yielding similar results.[35]

Table 5 shows the effects of the minimum pay law on total monthly hours spent

[34] An earlier literature on labor supply in taxi markets has raised the potential that taxi drivers stop working after hitting daily target earnings (Camerer et al., 1997; Farber, 2005, 2008; Thakral and Tô, 2021), although recent evidence from Buchholz, Shum and Xu (2025) suggests that what appears to be targeting behavior may in fact reflect the dynamics of search process and that labor supply elasticities are generally nonnegative.

[35] Details on the imputation method are provided in Appendix Section A.4. Appendix Table C.5 shows evidence that the results are robust to alternative imputation assumptions.

engaged with delivery apps. Columns (1)-(4) cover all apps, using the imputation, and columns (5)-(8) are restricted to DoorDash, for which on-app time is directly observed. As an additional proxy for time spent engaged with delivery apps, columns (9)-(12) study the number of days with delivery app activity during the month. Across all outcomes, and whether scaling outcomes in levels or logs, we find no evidence of substantial declines in hours worked. For more-attached workers, the point estimates are small (less than one hour in the level specification and less than 7 percent in the log specification) and statistically insignificant in all cases. Meanwhile, for less-attached workers, the point estimates are all positive and all but two are statistically insignificant. Across measures and specifications, there is no strong evidence for meaningful declines in hours as a result of the minimum pay policy, though the large standard errors limit the certainty with which we can rule out hours changes in either direction.

However, despite the lack of precision in our findings for hours, Table 6 documents a clear, statistically significant decline in the number of tasks completed per hour engaged with delivery apps. In columns (1)-(4), we find that drivers completed 0.3 to 0.4 fewer trips per active hour across both more- and less-attached drivers and for both hours measures. For Uber Eats and Grubhub, we observe information on each trip's request and end times and start and end locations, allowing us to calculate additional measures of drivers' time use and driving behavior. First, we calculate the utilization rate, defined as total monthly on-trip hours as a share of monthly active on-app hours, following Jacobs et al. (2024). Columns (5)-(6) estimate that utilization rates fell by 11 percentage points in the sample of more-attached drivers (17 percent of the post-period treatment mean utilization rate), and by 19 percentage points in the sample of less-attached drivers (29 percent of the post-period treatment mean utilization rate), implying that drivers incurred more unpaid time per completed delivery after the policy. Similarly, columns (7)-(8) report that unpaid wait time between tasks rose by roughly 5 minutes, which is a substantial increase relative to average wait times (6–7 minutes and 12–13 minutes for treatment drivers in the pre- and post-periods, respectively). While the sample of drivers for Uber Eats and Grubhub is limited and may not be representative of the full sample, we nonetheless find consistent evidence that the policy increased the share of their active time spent looking for tasks without being paid.[36]

If workers spend more time waiting for new delivery tasks, should one consider that to be leisure time or work time? On one hand, if workers are idle while waiting for new tasks, they may spend that time engaged in personal activities; moreover, they may save on gas and other driving-related expenses if they spend more time idle. Alternatively, if trips become harder to find, workers may incur monetary and time costs when driving to areas with relatively more delivery activity in search of offers.[37] We can partially test how drivers use their idle time between tasks by examining the effects on the average distance between the prior task's drop-off location and the subsequent task's pick-up location for trips on Uber Eats and Grubhub. The results, presented in columns (9) and (10) of Table 6, indicate that treated workers did in fact end up driving further between tasks after the implementation of the minimum pay standard. For more-attached workers, the straight line distance between tasks rose by 0.25–0.3 miles per task. These findings suggest that drivers continued to exert costly effort and incur vehicle-related costs during the additional idle time between trips, rather than consuming additional leisure time.

Taken together, the findings in this section show that as the number of trips per worker falls post-policy, there is no evidence that workers benefit from reduced working time, increased leisure, or reduced expenses associated with driving. Given the lack of monthly earnings increase after the first month post-policy, the evidence suggests that incumbent delivery drivers did not benefit from the minimum pay law in the long run. Instead, when the per-task pay rate increased, tasks became more scarce and drivers had to spend more time on-app searching for tasks, offsetting the increase in pay per task.

[36]In Appendix Table C.2, we document that longer waits between trips were not offset by shorter (more efficient) trips and were in part a reflection of a lower likelihood of being offered trips with batched tasks (single trips with multiple pick-ups/drop-offs). These changes in time use do not appear to be explained by changes in the times when drivers were out on the road; Appendix Figure B.16 shows no differential shift in the time distribution of tasks in the treatment region relative to the control region.

[37]Based on the authors' own experience driving on multiple apps, both the probability of getting an offer and the quality of offers improve continuously as one drives towards a central business district after dropping off a delivery in a residential neighborhood. During busy periods, attractive offers begin arriving sooner as one approaches the business district.

7 Discussion and Conclusion

Why did the minimum pay law fail to increase drivers' earnings and seemingly have minimal effect on their welfare? To help answer this question, in Appendix D we present a simple model of the app-based delivery market, closely following the model of the ride-sharing market introduced by Hall, Horton and Knoepfle (2023) and extended by Fisher (2024*a*). We assume that the gig delivery market is small in comparison to the rest of the labor market and allow for workers to have idiosyncratic preferences for working in each market. The variation in these preferences determines the labor supply elasticity to the delivery market and allows us to consider cases with frictional or free worker entry to the market. Rather than modeling the delivery platform's price setting problem as in Fisher (2024*a*), we make the simple but realistic assumption that increases in driver pay per task lead to a reduction in quantity of delivery tasks demanded. We then study how a regulation that raises the per-task pay rate affects the availability of tasks and drivers' earnings.

The key result is that a trip-level minimum pay standard will only increase individual drivers' earnings in settings where there is not free entry of drivers (i.e. a finite labor supply elasticity) and where the market-level elasticity of demand for drivers is sufficiently small (i.e. a demand elasticity with absolute value less than one). Intuitively, if the gig delivery market faces free entry of drivers, a binding minimum pay regulation will have no effect on drivers' per period earnings, as the equilibrium value of doing delivery work must always remain equal to the fixed, constant outside option available to all workers. In that case, driver entry will continue until the reduction in available tasks per worker precisely offsets the increase in pay per task. The condition on entry highlights a key tension in regulating gig work: increased pay necessitates the imposition of barriers to entry, which will tend to undermine the flexibility that the gig economy offers to workers.

The demand elasticity condition is more general—it is similar to the effect of a traditional minimum wage in a competitive labor market, in which a higher minimum wage lowers aggregate payroll if the absolute elasticity of demand is greater than one. However, in the traditional setting, there is a well-defined group of job holders who benefit from higher wages even when demand is highly elastic, with any reduction in demand manifesting in reduced employment levels. By contrast, in the platform gig-work setting where tasks are allocated across all job seekers, every *individual* experiences a reduction in expected pay when demand is sufficiently elastic.

Seen through the lens of the model, our finding of a lack of earnings growth suggests that at least one of these two conditions is not met. In the month following the regulation entering into force, we find that incumbent drivers saw an increase in monthly earnings, but those earnings subsequently fell back to their pre-treatment level, due to a reduction in the tasks available to incumbent drivers (Figures 7 and 8). We find no evidence that hours participating in gig delivery work fell substantially (Table 5), consistent with earnings returning to baseline due to drivers struggling to find delivery tasks. These results could be driven either by unit elastic demand for delivery drivers or free entry of drivers congesting the market.

An alternate possibility that we do not explicitly model is that platforms may have adjusted their fee structure and tipping policies to maximize political backlash to the reform. These responses may differ from what the optimal platform response would have been if considering the policy change as permanent. For instance, it is unclear why certain apps removed the option of tipping at checkout for deliveries in Seattle, which appears to have contributed to the decline in tips per delivery. While the intention may have been to soften the impact of higher fees on consumers, the change may have also been intended to reduce support for the policy among drivers. In the latter case, such decisions could be optimal for platforms if they increase the likelihood of subsequent repeal of the minimum pay ordinance, even if they were not profit-maximizing in the short run. In that event, the impacts we find may differ from the longer-term impacts that would occur if platforms were to accept the reforms as permanent.

However, our evidence suggests that free entry plays an important role in our setting, which significantly limits the scope for earnings gains, irrespective of the extent and causes of demand shifts. In particular, the descriptive evidence in Figure 5 shows that the *share* of tasks done by new entrants grows in Seattle relative to the rest of the state after the reform. While the number of overall deliveries falls in Seattle, the decline in deliveries completed by incumbents is disproportionately pronounced, indicating increased competition from new entrants even as the delivery market contracted.

These findings are of direct relevance to policymakers seeking to raise pay for gig delivery drivers. If the market for drivers is indeed subject to nearly free entry, minimum pay policies will struggle to improve drivers' earnings without imposing some form of entry barrier, such as those traditionally governing entry to non-platform taxi markets in many cities.[38] However, even with entry barriers, the qualitative effect of a per-task minimum pay regulation will depend on the elasticity of demand for delivery services. If that demand is elastic, a per-task minimum pay regulation will still reduce drivers' earnings even with barriers to entry.

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[38] In the case of free entry, the benefits of the minimum pay policy are dispersed throughout the broader labor market, of which the gig delivery market is a minuscule portion.

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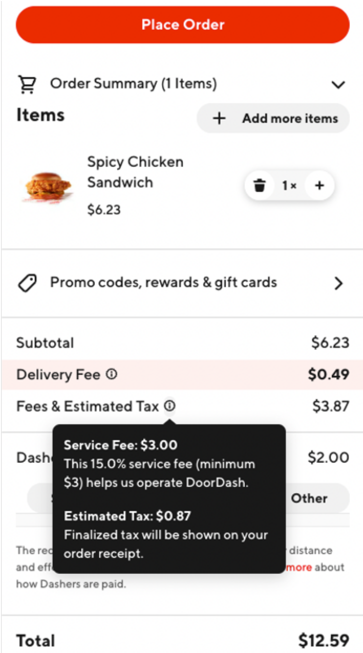
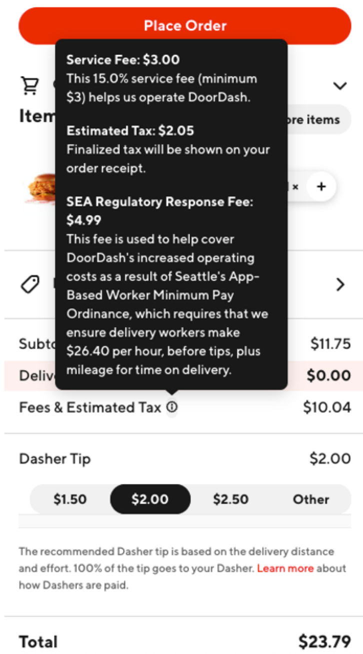
Figures

Figure 1: Fee Differences, by Region

(A) DoorDash, Spokane



(B) DoorDash, Seattle

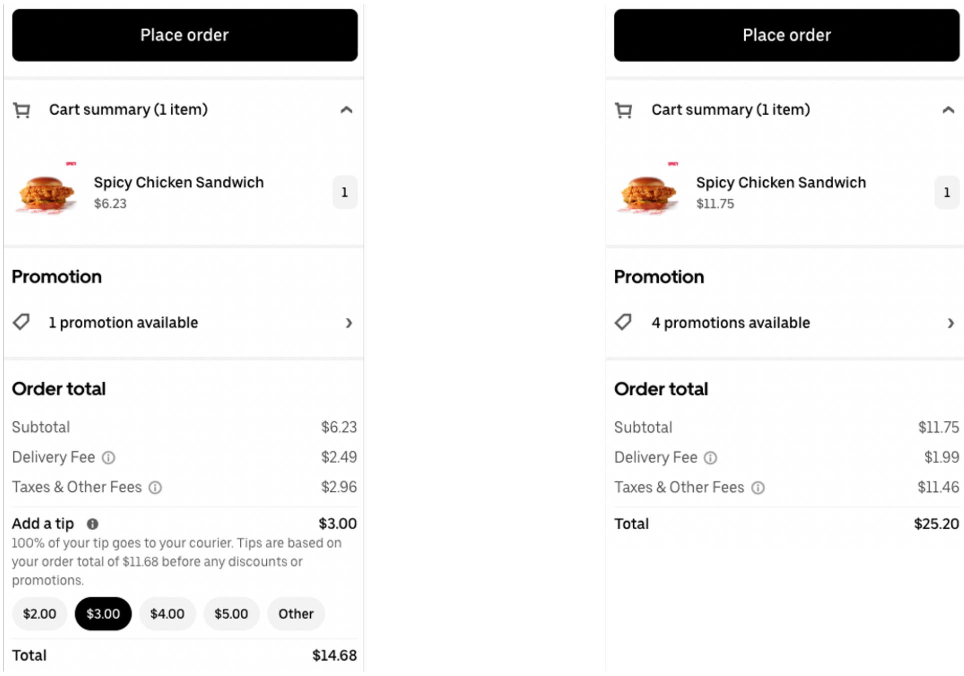


Notes: Panels (A) and (B) show screenshots of checkout pages for orders placed on the DoorDash app with shipping addresses in Spokane and Seattle, respectively. Text in the black boxes shows details of fees and estimated taxes of the respective orders. Screenshots recorded in the second half of 2024.

Figure 2: Tipping Differences, by Region

(A) Uber Eats, Spokane

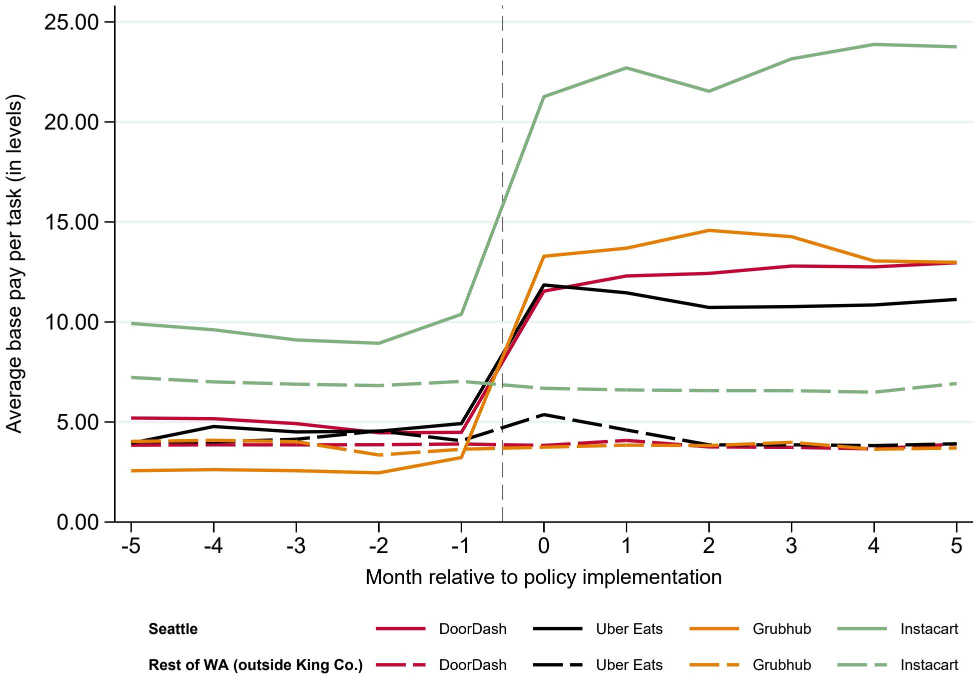
(B) Uber Eats, Seattle



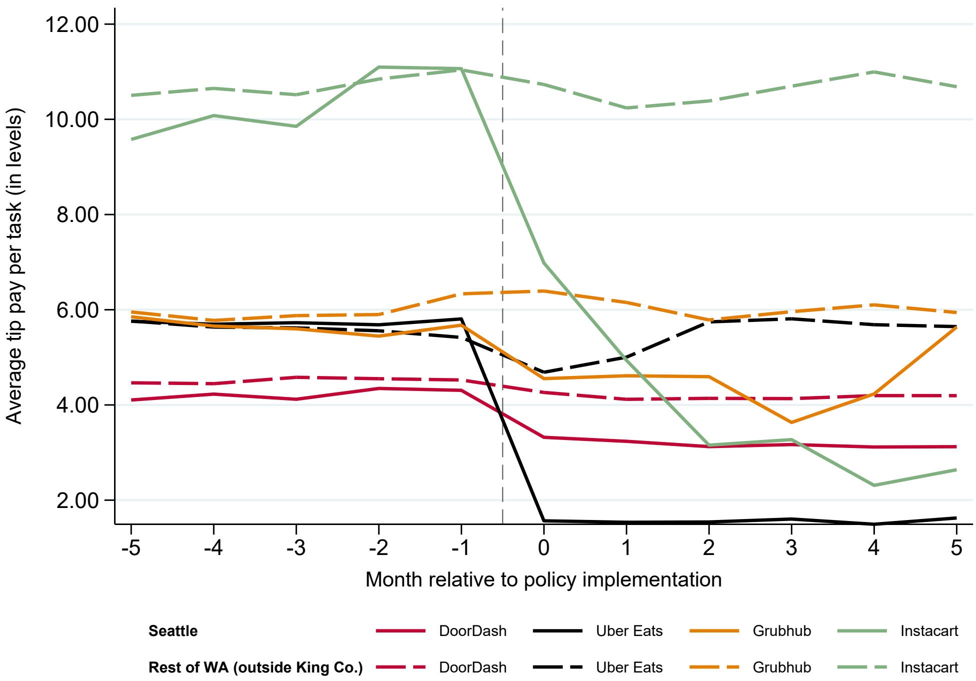
Notes: Panels (A) and (B) show screenshots of checkout pages for orders placed on the Uber Eats app with shipping addresses in Spokane and Seattle, respectively. In Panel (A), the section labeled "add a tip" provides customers the option to choose a tip amount; this section is absent for Seattle in Panel (B). Screenshots recorded in the second half of 2024.

Figure 3: Trends in Average Pay Per Task, by Delivery Platform and Region

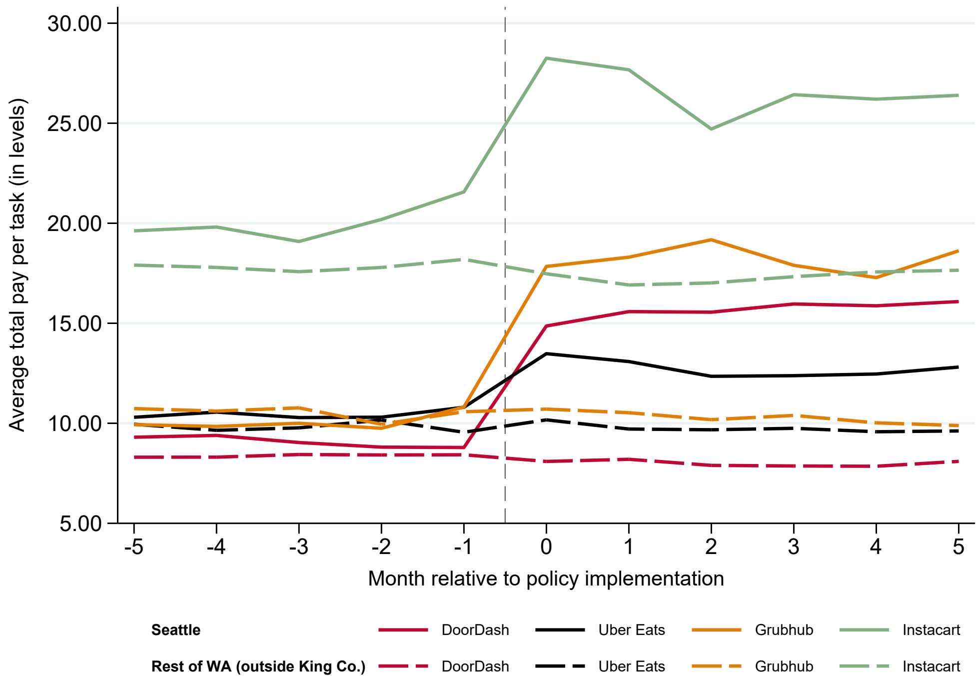
(A) Average Base Pay Per Task



(B) Average Tip Pay Per Task



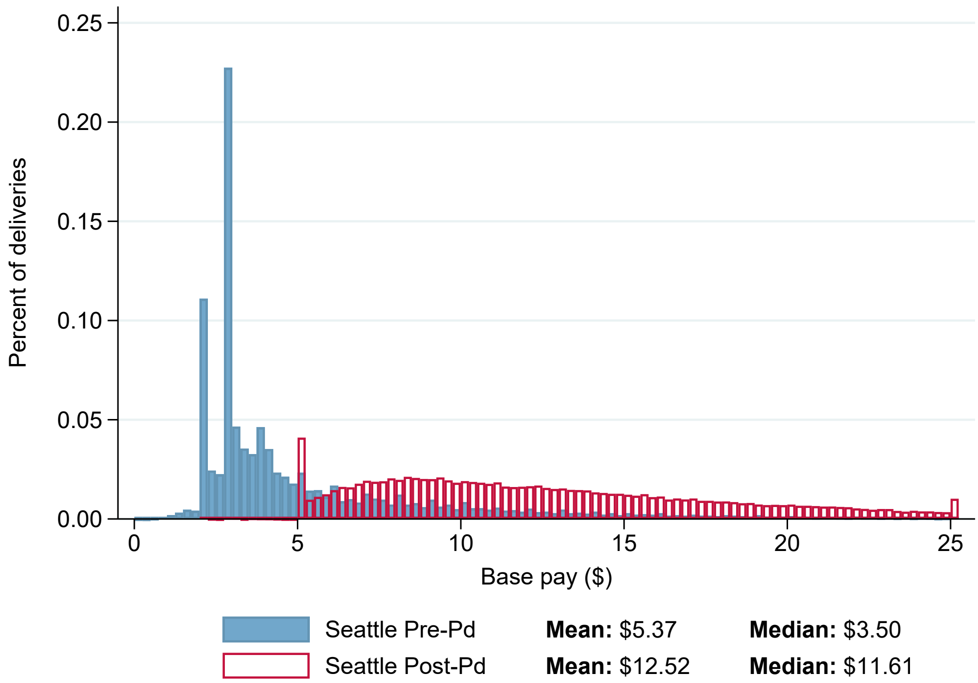
(C) Average Total Pay Per Task



Notes: Panel (A) plots the monthly trends in average base pay per task by delivery platform and region. Solid lines in red, black, orange, and green denote observations of delivery platforms DoorDash, Uber Eats, Grubhub, and Instacart, in Seattle, respectively. Dashed lines denote the observations of corresponding delivery platforms in Washington State outside King County. Month 0 denotes the first event month following ordinance implementation, and month -1 denotes the event month before the ordinance. Panels (B) and (C) present corresponding plots for average tip pay per task and average total pay per task, where total pay includes base pay, tip pay, and bonus pay.

Source: Authors' analysis of Gridwise data.

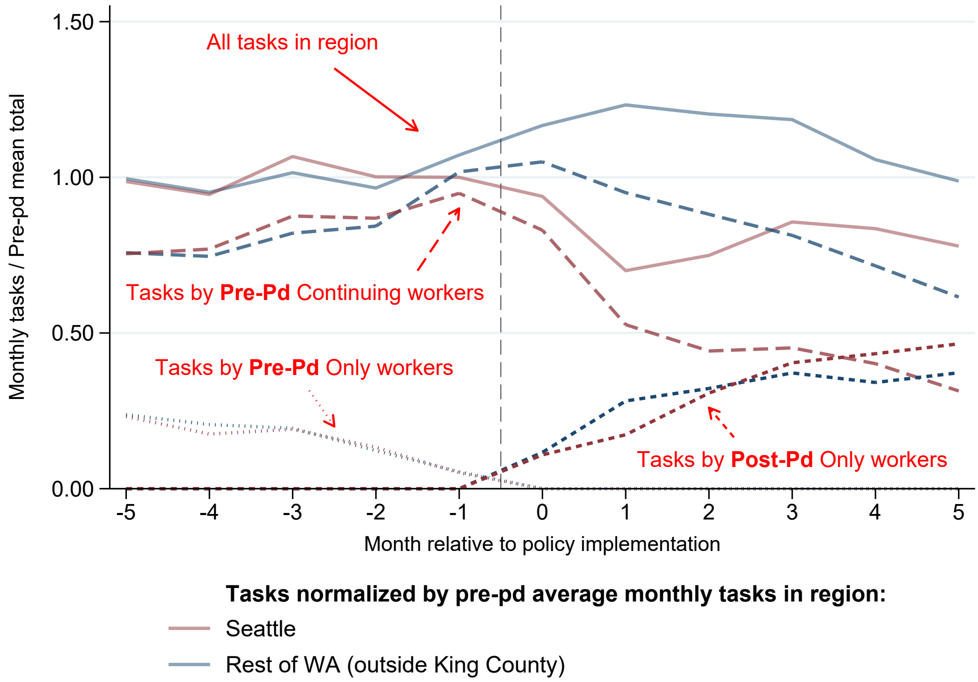
Figure 4: Distributions of Base Pay across Deliveries in Seattle, by Period



Notes: This figure presents the distributions of base pay across deliveries completed in Seattle in the pre-ordinance period and post-ordinance period, respectively. Blue solid bars denote the distribution of base pay across deliveries completed in Seattle in the pre-ordinance period. Red hollow bars denote the distribution of base pay across deliveries completed in Seattle in the post-ordinance period. Each bin represents $0.25. The figure omits observations above the 95th percentile of base pay across deliveries completed in Seattle for ease of visualization. Text on the right-hand side of the legend presents means and medians of base pay across deliveries completed in the pre-ordinance period and post-ordinance period in the sample.

Source: Authors' analysis of Gridwise data.

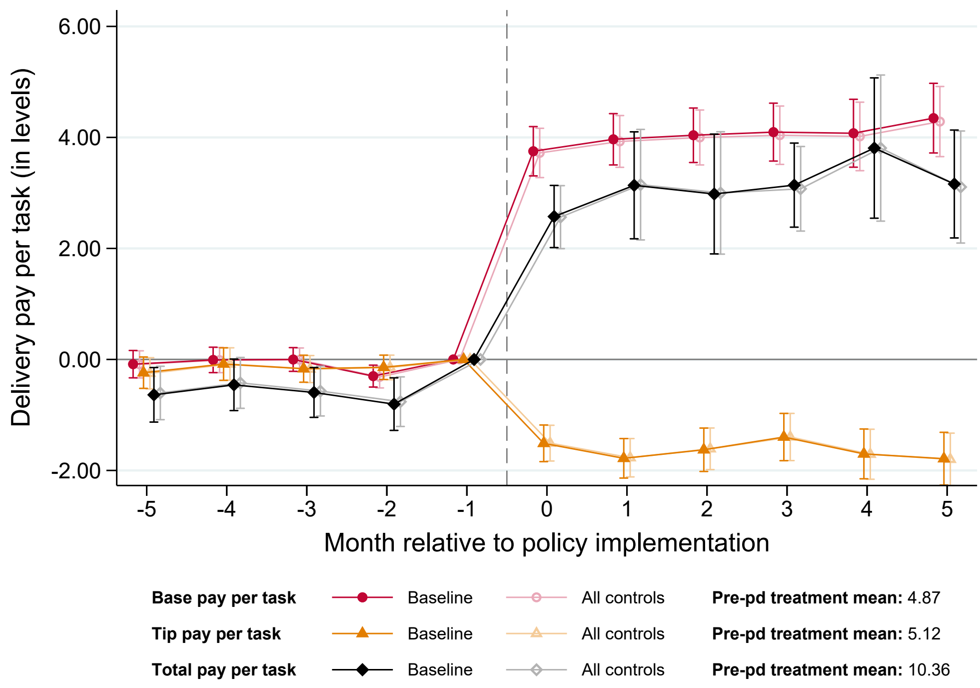
Figure 5: Trends in Delivery Tasks by All Workers, Incumbents, Workers Who Exit, and New Entrants, by Region



Notes: This figure plots the monthly trends in delivery tasks completed by all workers, continuing incumbents, workers who exit, and new entrants by region. All values are normalized by the pre-ordinance average monthly total delivery tasks completed by all workers in the respective regions. Solid lines in red and blue denote normalized monthly total delivery tasks completed by all workers in Seattle and Washington State outside King County, respectively. Longer-dashed lines denote normalized monthly delivery tasks completed by workers who were active in the pre-ordinance period and continued being active post-ordinance (continuing incumbents) in corresponding regions. Shorter-dashed lines denote normalized monthly delivery tasks completed by workers who were not active in the pre-ordinance period and only started to be active post-ordinance (new entrants) in corresponding regions. Faint dotted lines denote normalized monthly delivery tasks completed by workers who were active in the pre-ordinance period but became inactive post-ordinance (workers who exit) in corresponding regions. Month 0 denotes the first event month following ordinance implementation, and month -1 denotes the event month before the ordinance.

Source: Authors' analysis of Gridwise data.

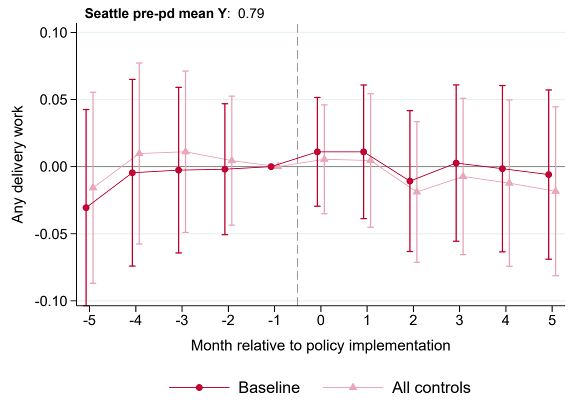
Figure 6: Effects on Delivery Pay Per Task



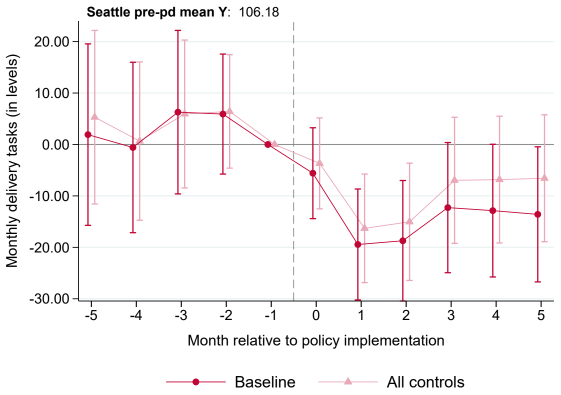
Notes: This figure plots estimates of β_k from Equation (1) using the sample of more-attached workers for three measures of delivery pay per task: delivery base pay per task (in red with circle markers), delivery tip pay per task (in orange with triangle markers), and delivery total pay per task (in black with diamond markers), where total pay includes base pay, tip pay, bonus pay, incentive pay, and adjustment pay. All outcomes are calculated in levels. Outcomes corresponding to zero delivery tasks in a given event month are coded as missing in that month. Month 0 denotes the first event month following ordinance implementation, and month -1 denotes the event month before the ordinance. Worker fixed effects and event year-month fixed effects are included in the estimation. Plots in darker colors with solid markers are estimates from Equation (1) without including additional controls (Baseline). Plots in lighter colors with hollow markers are estimates from Equation (1) including the full set of additional controls (All controls)—a set of pre-policy individual worker covariates interacted with indicators for event months. Covariates consist of delivery (rideshare) tasks, months active in delivery (rideshare) work, share of earnings from rideshare versus delivery work, and log total earnings, all measured during the pre-policy period. All samples are restricted to more-attached workers, defined as workers who performed delivery tasks above the median in the pre-policy period. The whiskers represent 95 percent confidence intervals. Standard errors are clustered at the worker level. Text on the right-hand side of the legend presents means of corresponding outcomes prior to the ordinance for exposed workers.

Figure 7: Effects on Delivery Tasks

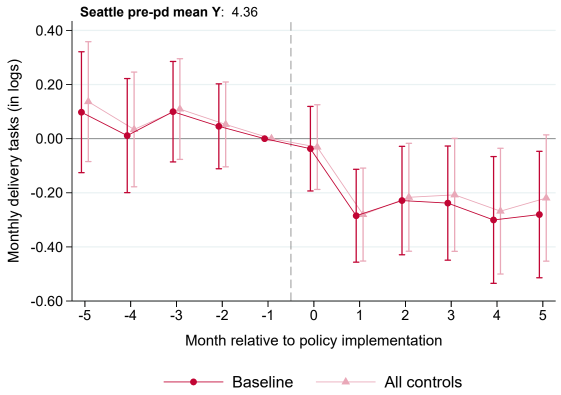
(A) Any



(B) Levels

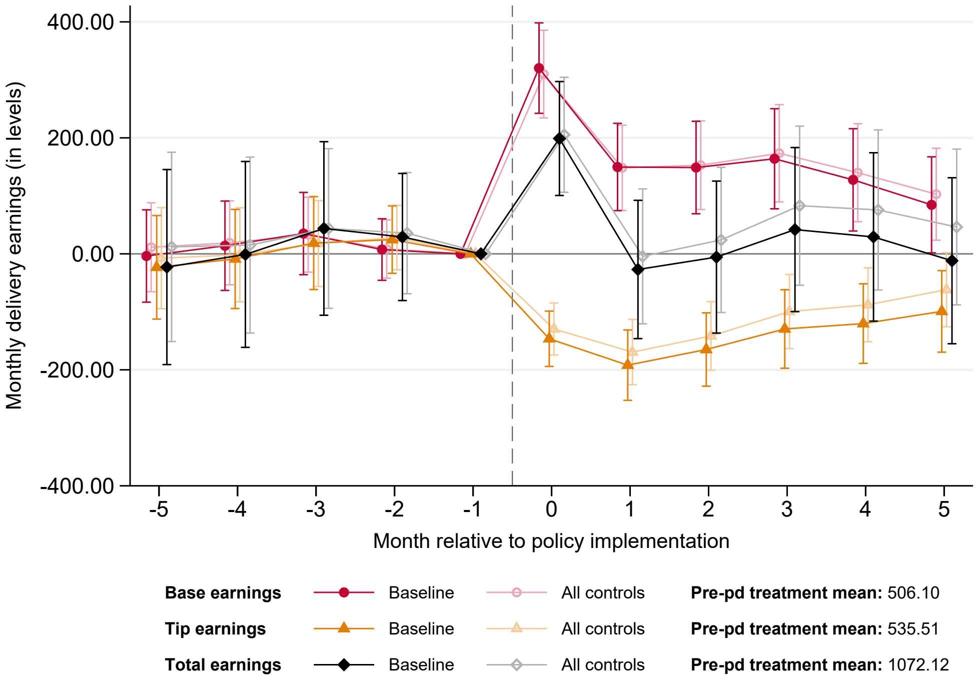


(C) Logs



Notes: Panels (A), (B), and (C) plot estimates of β_k from Equation (1) using the sample of more-attached workers for three measures of monthly completed delivery tasks: an indicator for performing any delivery tasks per month (Panel A), the number of monthly completed delivery tasks in levels (Panel B), and the number of monthly completed delivery tasks in logs (Panel C). Outcomes in levels are inclusive of zero values. Outcomes in logs corresponding to zero values in levels in a given event month are coded as missing in that month. Month 0 denotes the first event month following ordinance implementation, and month -1 denotes the event month before the ordinance. Worker fixed effects and event year-month fixed effects are included in the estimation. Plots in darker colors with circle markers are estimates from Equation (1) without including additional controls (Baseline). Plots in lighter colors with triangle markers are estimates from Equation (1) including the full set of additional controls (All controls)—a set of pre-policy individual worker covariates interacted with indicators for event months. Covariates consist of delivery (rideshare) tasks, months active in delivery (rideshare) work, share of earnings from rideshare versus delivery work, and log total earnings, all measured during the pre-policy period. All samples are restricted to more-attached workers, defined as workers who performed delivery tasks above the median in the pre-policy period. The whiskers represent 95 percent confidence intervals. Standard errors are clustered at the worker level. Text in the top left-hand corner in each panel presents the mean of the corresponding outcome prior to the ordinance for exposed workers.

Figure 8: Effects on Delivery Earnings



Notes: This figure plots estimates of β_k from Equation (1) using the sample of more-attached workers for three measures of monthly delivery earnings: monthly delivery base earnings (in red with circle markers), monthly delivery tip earnings (in orange with triangle markers), and monthly delivery total earnings (in black with diamond markers), where total earnings include base earnings, tip earnings, bonus earnings, incentive earnings, and adjustment earnings. All outcomes are calculated in levels, inclusive of zero values. Month 0 denotes the first event month following ordinance implementation, and month -1 denotes the event month before the ordinance. Worker fixed effects and event year-month fixed effects are included in the estimation. Plots in darker colors with solid markers are estimates from Equation (1) without including additional controls (Baseline). Plots in lighter colors with hollow markers are estimates from Equation (1) including the full set of additional controls (All controls)—a set of pre-policy individual worker covariates interacted with indicators for event months. Covariates consist of delivery (rideshare) tasks, months active in delivery (rideshare) work, share of earnings from rideshare versus delivery work, and log total earnings, all measured during the pre-policy period. All samples are restricted to more-attached workers, defined as workers who performed delivery tasks above the median in the pre-policy period. The whiskers represent 95 percent confidence intervals. Standard errors are clustered at the worker level. Text on the right-hand side of the legend presents means of corresponding outcomes prior to the ordinance for exposed workers.

Tables

Table 1: Descriptive Statistics

	Seattle workers		Non-Seattle workers		Difference	
	Mean	SD	Mean	SD	Δ	SE
	(1)	(2)	(3)	(4)	(5)	(6)
A. More-attached workers						
Number of delivery tasks (monthly average)	106.2	[99.6]	93.6	[83.4]	12.6*	(5.42)
Delivery earnings (monthly average)	1050.8	[991.1]	926.5	[859.2]	124.3*	(55.1)
Delivery earnings per task	9.93	[2.82]	10.1	[3.60]	-0.16	(0.21)
Delivery base earnings per task	4.81	[1.34]	4.40	[1.44]	0.41***	(0.086)
Delivery tip earnings per task	5.02	[1.93]	5.62	[2.69]	-0.60***	(0.15)
Months active in delivery	3.99	[1.28]	4.06	[1.20]	-0.069	(0.075)
Days active in delivery (monthly average)	12.5	[7.18]	12.6	[7.26]	-0.075	(0.44)
Rideshare earnings (monthly average)	77.3	[447.8]	70.9	[372.1]	6.42	(24.2)
N workers	377		908			
B. Less-attached workers						
Number of delivery tasks (monthly average)	5.99	[5.75]	5.61	[5.52]	0.38	(0.37)
Delivery earnings (monthly average)	62.0	[64.9]	58.5	[64.1]	3.50	(4.28)
Delivery earnings per task	11.6	[6.70]	11.2	[5.68]	0.35	(0.39)
Delivery base earnings per task	6.69	[6.45]	5.47	[3.51]	1.22***	(0.29)
Delivery tip earnings per task	4.77	[2.81]	5.63	[3.88]	-0.86***	(0.24)
Months active in delivery	1.98	[1.14]	2.07	[1.20]	-0.089	(0.079)
Days active in delivery (monthly average)	1.69	[1.72]	1.71	[1.62]	-0.015	(0.11)
Rideshare earnings (monthly average)	826.0	[1697.8]	138.9	[711.2]	687.1***	(68.0)
N workers	291		997			

Notes: This table reports means and standard deviations of key characteristics prior to the ordinance for Seattle workers (columns 1-2) and non-Seattle workers (columns 3-4), and estimated differences in characteristics and standard errors between the two groups (columns 5-6). Seattle (non-Seattle) workers refer to exposed (not-exposed) workers, defined as workers with intensity of exposure to Seattle above 80% (below 20%) as described in Section 3.2. Panels (A) and (B) calculate statistics for the samples of more-attached and less-attached workers, respectively, defined as workers who performed delivery tasks above and below the median in the pre-policy period. * $p < .05$, ** $p < .01$, *** $p < .001$.

Table 2: Effects on Delivery Pay Per Task

	Base pay per task		Tip pay per task		Bonus+incentive+adjustment pay per task		Total pay per task	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
A. More-attached workers								
Treated × Post	4.091***	4.050***	-1.506***	-1.498***	0.918***	0.909***	3.560***	3.521***
	(0.213)	(0.213)	(0.142)	(0.137)	(0.173)	(0.177)	(0.297)	(0.301)
N worker-months	9,906	9,906	9,906	9,906	9,906	9,906	9,906	9,906
N workers	1,265	1,265	1,265	1,265	1,265	1,265	1,265	1,265
Treatment pre-pd mean Y	4.877	4.877	5.128	5.128	0.330	0.330	10.365	10.365
Treatment post-pd mean Y	9.046	9.046	3.546	3.546	1.162	1.162	13.837	13.837
B. Less-attached workers								
Treated × Post	4.198***	4.468***	-1.595***	-1.579***	0.599	0.799	3.238***	3.713***
	(0.452)	(0.474)	(0.279)	(0.290)	(0.444)	(0.497)	(0.623)	(0.675)
N worker-months	4,160	4,160	4,160	4,160	4,160	4,160	4,160	4,160
N workers	941	941	941	941	941	941	941	941
Treatment pre-pd mean Y	6.445	6.445	4.811	4.811	0.494	0.494	11.793	11.793
Treatment post-pd mean Y	9.532	9.532	3.348	3.348	0.805	0.805	13.742	13.742
Controls		✓		✓		✓		✓

Notes: This table presents estimates of β from Equation (1) replacing the indicators for event months interacted with the term Treat_i with a single indicator for the post-ordinance period, for four measures of delivery pay per task: delivery base pay per task (columns 1-2), delivery tip pay per task (columns 3-4), delivery bonus, incentive, and adjustment pay per task (columns 5-6), and delivery total pay per task (columns 7-8), where total pay includes base pay, tip pay, bonus pay, incentive pay, and adjustment pay. All outcomes are calculated in levels. Outcomes corresponding to zero delivery tasks in a given event month are coded as missing in that month. Worker fixed effects and event month fixed effects are included in the estimation. Columns 1, 3, 5, and 7 present estimates without including additional controls, and columns 2, 4, 6, and 8 present estimates including the full set of additional controls—a set of pre-policy individual worker covariates interacted with the indicator for the post-ordinance period. Covariates consist of delivery (rideshare) tasks, months active in delivery (rideshare) work, share of earnings from rideshare versus delivery work, and log total earnings, all measured during the pre-policy period. Panels (A) and (B) present estimates for the samples of more-attached and less-attached workers, respectively, defined as workers who performed delivery tasks above and below the median in the pre-policy period. Standard errors in parentheses are clustered at the worker level. The "treatment pre-period mean Y" is the mean of the corresponding outcome prior to the ordinance for exposed workers, while the "treatment post-period mean Y" reports means after the ordinance for exposed workers. * $p < .05$, ** $p < .01$, *** $p < .001$.

Table 3: Effects on Delivery Tasks

	Any		Levels		Logs	
	(1)	(2)	(3)	(4)	(5)	(6)
A. More-attached workers						
Treated × Post	0.009	-0.010	-16.450**	-12.891**	-0.262***	-0.261***
	(0.024)	(0.022)	(5.163)	(4.341)	(0.066)	(0.065)
N worker-months	14,135	14,135	14,135	14,135	9,906	9,906
N workers	1,285	1,285	1,285	1,285	1,265	1,265
Treatment pre-pd mean Y	0.799	0.799	106.181	106.181	4.356	4.356
Treatment post-pd mean Y	0.610	0.610	62.031	62.031	3.884	3.884
B. Less-attached workers						
Treated × Post	0.006	-0.021	-0.466	-0.779	0.069	0.166
	(0.022)	(0.021)	(1.276)	(1.414)	(0.132)	(0.128)
N worker-months	14,168	14,168	14,168	14,168	4,160	4,160
N workers	1,288	1,288	1,288	1,288	941	941
Treatment pre-pd mean Y	0.396	0.396	5.994	5.994	2.117	2.117
Treatment post-pd mean Y	0.233	0.233	7.548	7.548	2.546	2.546
Controls		✓		✓		✓

Notes: This table presents estimates of β from Equation (1) replacing the indicators for event months interacted with the term Treat$_i$ with a single indicator for the post-ordinance period, for three measures of monthly completed delivery tasks: an indicator for performing any delivery tasks per month (columns 1-2), the number of monthly completed delivery tasks in levels (columns 3-4), and the number of monthly completed delivery tasks in logs (columns 5-6). Outcomes in levels are inclusive of zero values. Outcomes in logs corresponding to zero values in levels in a given event month are coded as missing in that month. Worker fixed effects and event year-month fixed effects are included in the estimation. Columns 1, 3, and 5 present estimates without including additional controls, and columns 2, 4, and 6 present estimates including the full set of additional controls—a set of pre-policy individual worker covariates interacted with the indicator for the post-ordinance period. Covariates consist of delivery (rideshare) tasks, months active in delivery (rideshare) work, share of earnings from rideshare versus delivery work, and log total earnings, all measured during the pre-policy period. Panels (A) and (B) present estimates for the samples of more-attached and less-attached workers, respectively, defined as workers who performed delivery tasks above and below the median in the pre-policy period. Standard errors in parentheses are clustered at the worker level. The "treatment pre-period mean Y" is the mean of the corresponding outcome prior to the ordinance for exposed workers, while the "treatment post-period mean Y" reports means after the ordinance for exposed workers. * $p < .05$, ** $p < .01$, *** $p < .001$.

Table 4: Effects on Delivery Earnings

	Base earnings		Tip earnings		Bonus+incentive+adjustment earnings		Total earnings	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
A. More-attached workers								
Treated × Post	155.311***	156.893***	-144.256***	-122.330***	17.514***	16.553***	27.768	50.346
	(35.658)	(33.994)	(27.643)	(22.138)	(5.249)	(4.945)	(56.732)	(51.494)
N worker-months	14,135	14,135	14,135	14,135	14,135	14,135	14,135	14,135
N workers	1,285	1,285	1,285	1,285	1,285	1,285	1,285	1,285
Treatment pre-pd mean Y	506.110	506.110	535.518	535.518	28.364	28.364	1072.123	1072.123
Treatment post-pd mean Y	548.647	548.647	224.643	224.643	42.486	42.486	817.126	817.126
B. Less-attached workers								
Treated × Post	33.223**	32.619**	-14.247**	-15.193*	3.779***	3.801**	22.619	21.064
	(10.788)	(11.881)	(5.292)	(5.966)	(1.030)	(1.235)	(15.795)	(17.524)
N worker-months	14,168	14,168	14,168	14,168	14,168	14,168	14,168	14,168
N workers	1,288	1,288	1,288	1,288	1,288	1,288	1,288	1,288
Treatment pre-pd mean Y	30.911	30.911	30.467	30.467	1.890	1.890	63.477	63.477
Treatment post-pd mean Y	71.701	71.701	24.730	24.730	4.549	4.549	101.034	101.034
Controls		✓		✓		✓		✓

Notes: This table presents estimates of β from Equation (1) replacing the indicators for event months interacted with the term Treat_i with a single indicator for the post-ordinance period, for four measures of monthly delivery earnings: monthly delivery base earnings (columns 1-2), monthly delivery tip earnings (columns 3-4), monthly delivery bonus, incentive, and adjustment earnings (columns 5-6), and monthly delivery total earnings (columns 7-8), where total earnings are reported directly in the data and include base earnings, tip earnings, bonus earnings, incentive earnings, and adjustment earnings (even if the individual components are not separately reported in the data). All outcomes are calculated in levels, inclusive of zero values. Worker fixed effects and event year-month fixed effects are included in the estimation. Columns 1, 3, 5, and 7 present estimates without including additional controls, and columns 2, 4, 6, and 8 present estimates including the full set of additional controls—a set of pre-policy individual worker covariates interacted with the indicator for the post-ordinance period. Covariates consist of delivery (rideshare) tasks, months active in delivery (rideshare) work, share of earnings from rideshare versus delivery work, and log total earnings, all measured during the pre-policy period. Panels (A) and (B) present estimates for the samples of more-attached and less-attached workers, respectively, defined as workers who performed delivery tasks above and below the median in the pre-policy period. Standard errors in parentheses are clustered at the worker level. The "treatment pre-period mean Y" is the mean of the corresponding outcome prior to the ordinance for exposed workers, while the "treatment post-period mean Y" reports means after the ordinance for exposed workers. * $p < .05$, ** $p < .01$, *** $p < .001$.

Table 5: Effects on Delivery Hours and Active Delivery Days

	Total hours, with imputation				DoorDash hours				Total active days			
	Levels		Logs		Levels		Logs		Levels		Logs	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
A. More-attached workers												
Treated × Post	-0.952	0.170	-0.067	-0.069	-0.881	-0.056	0.026	0.020	0.296	0.223	-0.014	-0.021
	(2.705)	(2.449)	(0.065)	(0.064)	(2.278)	(2.133)	(0.085)	(0.085)	(0.451)	(0.421)	(0.044)	(0.044)
N worker-months	14,135	14,135	9,905	9,905	14,135	14,135	6,388	6,388	14,135	14,135	9,906	9,906
N workers	1,285	1,285	1,265	1,265	1,285	1,285	880	880	1,285	1,285	1,265	1,265
Treatment pre-pd mean Y	54.380	54.380	3.709	3.709	34.687	34.687	3.527	3.527	11.742	11.742	2.443	2.443
Treatment post-pd mean Y	38.185	38.185	3.420	3.420	26.352	26.352	3.424	3.424	8.637	8.637	2.320	2.320
B. Less-attached workers												
Treated × Post	0.864	0.782	0.235	0.345**	0.588	0.725	0.160	0.224	0.337	0.273	0.132	0.210*
	(0.793)	(0.876)	(0.134)	(0.134)	(0.590)	(0.668)	(0.213)	(0.200)	(0.234)	(0.243)	(0.093)	(0.094)
N worker-months	14,168	14,168	4,160	4,160	14,168	14,168	1,493	1,493	14,168	14,168	4,160	4,160
N workers	1,288	1,288	941	941	1,288	1,288	371	371	1,288	1,288	941	941
Treatment pre-pd mean Y	3.336	3.336	1.501	1.501	1.679	1.679	1.812	1.812	1.531	1.531	1.064	1.064
Treatment post-pd mean Y	5.030	5.030	2.114	2.114	2.803	2.803	2.320	2.320	1.826	1.826	1.556	1.556
Controls		✓		✓		✓		✓		✓		✓

Notes: This table presents estimates of β from Equation (1) replacing the indicators for event months interacted with the term Treat_i with a single indicator for the post-ordinance period, for three measures of monthly delivery hours and active delivery days: imputed monthly total hours on all delivery platforms (columns 1-2 for levels and columns 3-4 for logs), monthly total hours on DoorDash (columns 5-6 for levels and columns 7-8 for logs), and monthly active days on all delivery platforms (columns 9-10 for levels and columns 11-12 for logs). Outcomes in levels are inclusive of zero values. Outcomes in logs corresponding to zero values in levels in a given event month are coded as missing in that month. Worker fixed effects and event year-month fixed effects are included in the estimation. Columns 1, 3, 5, 7, 9, and 11 present estimates without including additional controls, and columns 2, 4, 6, 8, 10, and 12 present estimates including the full set of additional controls—a set of pre-policy individual worker covariates interacted with the indicator for the post-ordinance period. Covariates consist of delivery (rideshare) tasks, months active in delivery (rideshare) work, share of earnings from rideshare versus delivery work, and log total earnings, all measured during the pre-policy period. Panels (A) and (B) present estimates for the samples of more-attached and less-attached workers, respectively, defined as workers who performed delivery tasks above and below the median in the pre-policy period. Standard errors in parentheses are clustered at the worker level. The "treatment pre-period mean Y" is the mean of the corresponding outcome prior to the ordinance for exposed workers, while the "treatment post-period mean Y" reports means after the ordinance for exposed workers. * $p < .05$, ** $p < .01$, *** $p < .001$.

Table 6: Effects on Delivery Time Use

	Tasks per hour				Utilization rate		Task wait time (min)		Distance between tasks (mi)	
	All apps, with imputation		DoorDash only							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
A. More-attached workers										
Treated × Post	-0.340***	-0.333***	-0.340***	-0.341***	-0.109***	-0.108***	5.415***	5.363***	0.274***	0.296***
	(0.027)	(0.026)	(0.037)	(0.036)	(0.023)	(0.022)	(0.601)	(0.622)	(0.069)	(0.071)
N worker-months	9,905	9,905	6,388	6,388	3,727	3,727	4,461	4,461	4,377	4,377
N workers	1,265	1,265	880	880	591	591	656	656	644	644
Treatment pre-pd mean Y	1.981	1.981	1.970	1.970	0.759	0.759	6.920	6.920	1.464	1.464
Treatment post-pd mean Y	1.666	1.666	1.620	1.620	0.637	0.637	13.521	13.521	1.763	1.763
B. Less-attached workers										
Treated × Post	-0.278***	-0.333***	-0.420***	-0.411***	-0.193**	-0.193**	5.250***	4.251**	0.194	0.099
	(0.072)	(0.070)	(0.098)	(0.092)	(0.064)	(0.067)	(1.359)	(1.454)	(0.194)	(0.233)
N worker-months	4,160	4,160	1,493	1,493	1,453	1,453	1,549	1,549	1,523	1,523
N workers	941	941	371	371	365	365	386	386	378	378
Treatment pre-pd mean Y	1.974	1.974	1.914	1.914	0.882	0.882	5.663	5.663	1.440	1.440
Treatment post-pd mean Y	1.652	1.652	1.465	1.465	0.666	0.666	12.661	12.661	1.778	1.778
Controls		✓		✓		✓		✓		✓

Notes: This table presents estimates of β from Equation (1) replacing the indicators for event months interacted with the term Treat$_i$ with a single indicator for the post-ordinance period, for four measures of monthly time use in delivery: the monthly average number of delivery tasks completed per hour (columns 1-2 for all delivery platforms using imputed monthly total hours, and columns 3-4 for DoorDash which reports hours directly), the monthly utilization rate (columns 5-6), monthly average wait time (minutes) per task (columns 7-8), and monthly average miles traveled between tasks (columns 9-10). Utilization rates, task wait times, and distances between tasks are calculated for Uber Eats and Grubhub, given data availability. All outcomes are calculated in levels. Outcomes corresponding to zero tasks completed on the relevant delivery platform(s) in a given event month are coded as missing in that month. Worker fixed effects and event year-month fixed effects are included in the estimation. Columns 1, 3, 5, 7, and 9 present estimates without including additional controls, and columns 2, 4, 6, 8, and 10 present estimates including the full set of additional controls—a set of pre-policy individual worker covariates interacted with the indicator for the post-ordinance period. Covariates consist of delivery (rideshare) tasks, months active in delivery (rideshare) work, share of earnings from rideshare versus delivery work, and log total earnings, all measured during the pre-policy period. Panels (A) and (B) present estimates for the samples of more-attached and less-attached workers, respectively, defined as workers who performed delivery tasks above and below the median in the pre-policy period. Standard errors in parentheses are clustered at the worker level. The "treatment pre-period mean Y" is the mean of the corresponding outcome prior to the ordinance for exposed workers, while the "treatment post-period mean Y" reports means after the ordinance for exposed workers. * $p < .05$, ** $p < .01$, *** $p < .001$.

Online Appendices [Not for Publication]

A Data Appendix

This appendix describes the Gridwise data, our data cleaning process, and how we construct the main sample and variables used in our analysis. As described in Section 3.1, Gridwise Inc. provides a task-level gig work dataset that tracks gig workers' activity and earnings across platforms over time. These data are collected through the Gridwise app, which aids users in tracking and optimizing their gig work earnings, expenses, and profitability. Users link gig work platform apps to the Gridwise app, which automatically collects users' gig activity and earnings data.

A.1 Data Files

We begin with raw data obtained from Gridwise Inc. consisting of all activities by Gridwise app users in Washington State between August 1, 2023 and July 31, 2024. We received three data files from Gridwise. The main data file records activity and earnings information for each unit of work ("activity" hereafter) on major delivery and rideshare platforms including DoorDash, Grubhub, Instacart, Uber Eats, Lyft, and Uber. The reporting unit of work varies by platform. For Grubhub, it is a single task. For Instacart, Uber Eats, Lyft, and Uber, it is either a single task or a batch of tasks completed in a single trip. For DoorDash, it is a shift, typically containing multiple tasks and/or batches. Each observation includes a unique worker ID and platform ID, the date and time when the activity request was accepted by the worker, when the activity started, and when the activity ended, the locations where the activity started and ended, the number of tasks encompassed, and payments to the worker. Date and time are recorded in Coordinated Universal Time (UTC) to the second, and location is reported at the census block level. The total payments are broken down into base payment, bonus, and tip.

The second data file complements the main activity-level dataset with a task-level breakdown for platforms where an activity represents a shift or a batch. In this dataset, each observation represents a single task. Similar to the activity-level data, for each task we observe the worker ID and platform ID, the start and end date, time, and location, payments to the worker, and also the activity ID and batch ID. However, the location recorded in the task-level data is at the less granular Core Based Statistical Area (CBSA) level, which does not allow us to distinguish whether a task started or ended within the city of Seattle. Also, for batched tasks, we only observe the start and end date and time for the whole batch but not for individual tasks. For DoorDash tasks, we only observe start date and time but not end date and time.

Our analysis focuses on the main activity-level dataset because of its detailed location information and completeness for date and time. We incorporate the task-level dataset only when needed to construct relevant variables and note when this is the case.

The third data file we received provides additional platform-implemented adjustment and incentive payment information. Platforms frequently offer several types of promotional pay opportunities to drivers. This can result in driver pay being modified at the end of a pay period in order to comply with the promotion. Adjustments promise a certain amount of pay at the end of a driver's time worked. Platforms pay the drivers as they normally would have, and then after the fact verify that the promised pay was met. Compliance with a minimum wage would therefore may be implemented by platforms in the form of an adjustment if the initial payment failed to meet the minimum pay standard. Incentives, on the other hand, represent a sort of bonus, where completing some set of tasks results in extra payment from the platform. For example, DoorDash may offer an extra $100 to any driver who completes a certain number of deliveries in that month. These promotions are not associated with a single trip or shift; rather, they apply to some period of time during which any trips or shifts completed were subject to the adjustment or incentive. In this file, each observation records an adjustment and/or incentive payment to a worker, which includes the worker ID, platform ID, start date and time, starting geographic region (CBSA), and adjustment and incentive payment amounts. When incorporating adjustment and incentive payments in our analysis, we merge them with other pay data at the worker-month level. Note that workers may receive adjustment and incentive pay from anywhere in the country where they have worked, so we only include adjustment and incentive pay associated with Washington State or immediately adjacent states—Idaho and Oregon. These three states comprise 95.6% of the adjustment earnings and 94.8% of the incentive earnings made by workers in our analysis sample when using the continuous treatment definition (see below).[39]

A.2 Data Cleaning and Imputation

We begin by imputing missing values for request time and end time. In the main activity-level dataset, for entries with missing request time we use the start time. This imputation applies to 52% of the delivery activity entries, including all DoorDash and Instacart activities, 0.6% of the Grubhub activities, and none of the Uber Eats activities. For entries with missing end time, we use the start time if available and the request time otherwise. This imputation applies to only 0.02% of the delivery activities, including 0.56% of Grubhub activities and none of the DoorDash or Uber Eats activities. We assign each activity the date of its end time, since few end times are missing in the raw data.

In our main activity-level dataset, all delivery observations report both total pay and base pay. However, in some cases tip pay is missing (6.9%, including 16% of the Uber Eats activities and none of the Grubhub or DoorDash activities) and in many cases bonus pay is missing (44%, including 15% of the DoorDash activities, 86% of the Uber Eats activities, and none of the Grubhub activities). We address these missing values in two ways. In the main analysis, we directly use the total pay values recorded in the raw data. Then as a robustness check, we impute missing tip and bonus pay values with zeros and then calculate total pay as the sum of the base, tip, and bonus pay (Appendix Tables C.3 and C.4). These two methods for calculating total pay fall within one dollar of each other for 99.69% of delivery activity observations. We use a similar strategy for tips and bonus pay; in the main analysis, missing tip and bonus values are treated as zeros, and as a robustness check we impute tip (bonus) pay by subtracting all other pay components from the recorded total pay after imputing missing bonus (tip) pay values with zeros.

Finally, we address a small number of observations with implausibly long trips or shifts. In our main activity-level dataset several Instacart trips and DoorDash shifts (<0.1%) report lasting more than a day, so we winsorize the durations of Instacart trips and the last trip within a DoorDash shift to the respective 99.9th percentile. We winsorize

[39] Among these workers, for adjustment earnings, 93.4% are from Washington, 13.9% are from Oregon, and 2.1% are from Idaho. For incentive earnings, 92.6% are from Washington, 17.8% are from Oregon, and 2% are from Idaho. These numbers do not add up to the totals because many observations are associated with multiple states (e.g., Washington-Oregon) and we only have the CBSA-level geographic information available in the incentives and adjustments file.

only the last trip in a DoorDash shift because the last trip usually drives the extremely long shift duration (likely due to workers forgetting to close the shift in the DoorDash app).

We next convert all time variables from UTC to Pacific time, adjusting for daylight savings. In our analysis, we define months as periods from the 13th of the calendar month to the 12th of the following month, to align with January 13, 2024 when the Seattle minimum-pay ordinance went into effect. We restrict attention to five months before and six months after the ordinance implementation date, reflecting the time coverage of our sample.

A.3 Sample Construction

In our main individual analysis, we consider the sample of workers who were active in delivery work during the pre-ordinance period. In particular, we keep workers with at least one delivery activity prior to ordinance implementation. This includes 3,774 workers completing 2,036,682 tasks and earning a total of $23,608,818 between August 13, 2023 and July 12, 2024. We separate these workers into two groups by their pre-ordinance degrees of attachment to the delivery market. Workers who performed above the median number of delivery tasks (19.8 tasks for the binary treatment sample and 20.4 for the continuous treatment sample—see below for details) are defined as more-attached, while those below the median are less-attached. We focus primarily on the more-attached sample but present all findings for both groups.

In the descriptive analysis of aggregate market trends, we consider the sample of all delivery activities in the data (not just those done by pre-ordinance active workers in our individual analysis sample) that start or end in the city of Seattle or start and end outside of King County (which contains Seattle), omitting those primarily in the King County market outside Seattle or with missing start or end location information. This includes 918,223 tasks completed by 3,831 workers between August 13, 2023 and July 12, 2024, generating a total of $9,886,838 in worker earnings.

A.4 Variable Construction

Exposure to work in Seattle. We calculate a worker's exposure to delivery work in Seattle based on the worker's share of pre-ordinance delivery earnings coming from activities in Seattle. We first classify each delivery activity into a geographic group based on the start and end location. Activities that start or end in the city of Seattle are classified as Seattle activities (denoted as S).[40] Activities that start and end outside of King County (which contains Seattle) are classified as non-Seattle activities (denoted as N). All the remaining activities, including those starting (ending) in other parts of King County outside Seattle and ending (starting) outside the city of Seattle and those with missing start or end location information, are classified as residual activities (denoted as R).[41] Appendix Figure B.5 shows visual examples of the three geographic groups. We use the U.S. 2020 Census Block to Map Sheet relationship file to classify activities into

[40] See https://library.municode.com/wa/seattle/codes/municipal_code?nodeId=TIT8LAST_CH8.37ASEWOMIPA_8.37.030ASEWOCO (accessed September 24, 2025).

[41] For activities that represent a single task or a batch, we reclassify activities starting (ending) in other parts of King County outside Seattle and ending (starting) outside King County as non-Seattle activities, given that single tasks or batches around the boundaries of the large King County are unlikely to pass through the city of Seattle.

the three geographic groups based on the block-level location information provided in the data.[42] Next we calculate exposure to delivery work in Seattle as $S/(S+N)$, the share of *classifiable* pre-ordinance delivery earnings coming from Seattle activities. Residual activities (R) are excluded from the exposure calculation to enhance the credibility that individuals with zero measured exposure did not drive for delivery work in Seattle in the pre-ordinance period.[43] In the main analysis, we use a discretized version of the exposure measure in which workers with measured exposure of 80% or greater are coded as exposed ("treated") and those with measured exposure of 20% or less are coded as not-exposed ("control"). Those with measured exposure between 20% and 80% are omitted from the main analysis. In robustness tests, we use the continuous exposure measure and include the full sample of workers with non-missing measured exposure.

Monthly delivery tasks. For each worker, we construct an indicator for performing any delivery tasks in a month, and calculate the number of delivery tasks completed in each month. Individuals who do not appear in the data for a given month are coded as not performing any delivery tasks and completing zero delivery tasks in that month.

Monthly delivery earnings. We calculate a worker's monthly delivery base earnings, tip earnings, bonus and incentive earnings, adjustment earnings, and total earnings, which are the sum of the four components just mentioned. Individuals who do not appear in the data for a given month are coded as earning zero for all four components and the total.

Monthly average delivery pay per task. For each worker-month we calculate the per-task average delivery base pay, tip pay, bonus, incentive, and adjustment pay, and total pay, calculated as the relevant pay amount divided by the total number of delivery tasks in that month. Individuals who do not appear in the data or have no observations of delivery activities for a given month are excluded from the average pay per task analyses for that month.

Monthly delivery hours and active delivery days. For each worker, we calculate the monthly number of hours of delivery work, hours of DoorDash work, and days active in delivery work. The empirical challenge for calculating hours of delivery work is that we do not consistently observe the total time that workers spend engaged in delivery work across platforms in our data. Ideally, we would observe both revenue-generating hours and unpaid hours spent waiting or driving in search of new tasks on each platform. However, in our data, we observe total on-app time (the best available measurement of total hours worked) only for DoorDash. For other delivery platforms, we only observe time on revenue-generating activities—the duration of an individual trip—but not active time between trips. We therefore impute incomplete time information using the following procedure.

We first impute hours worked for non-DoorDash delivery activities. We define a "shift" as a sequence of consecutive non-DoorDash delivery trips for a given worker with

[42] We choose to classify activities rather than individual tasks into geographic groups because some tasks in our data, specifically tasks in shifts or batches, only have CBSA-level start and end location information that does not allow us to sort them into the three geographic groups.

[43] By construction, individuals who only made pre-ordinance delivery earnings from residual activities have missing measured exposure and are not included in the sample.

between-trip gaps no longer than one hour.[44] For multi-trip shifts, we impute the total work hours based on the trip-level end time information (which is nearly always available) using the following expression:

$$(End\ Time_{last\ trip\ in\ shift} - End\ Time_{first\ trip\ in\ shift}) \times \frac{N}{N-1}, \tag{2}$$

where $(End\ Time_{last\ trip\ in\ shift} - End\ Time_{first\ trip\ in\ shift})$ measures the time between the end of the first and last trip in the shift, and N is the number of trips in the shift. We then aggregate DoorDash shifts (for which hours are directly observed) and non-DoorDash shifts (with imputed hours) if they fall within one hour of each other using a similar imputation procedure. For non-DoorDash shifts that contain only one trip, we impute their hours as the average observed in DoorDash (for which hours are observed) and for multi-trip non-DoorDash shifts (imputed as just described), separately for Seattle workers and non-Seattle workers. In robustness tests, we show results multiplying the imputed hours by 0.75 and 1.25. We then calculate total monthly hours for each worker. Individuals who do not appear in the data for a given month are coded as working zero hours of delivery work in that month. Because the DoorDash observations provide the most reliable hours information, without requiring imputation, we present hours results for DoorDash alone as a robustness test.

We calculate the monthly number of days active in delivery work as the number of days in the month for which the worker has at least one delivery activity. Individuals who do not appear in the data for a given month are coded as having zero days active in delivery work in that month.

Monthly delivery time use. For each worker, we calculate the monthly average number of delivery tasks completed per hour, utilization rate, average wait time per task, and average distance between tasks for delivery work. We calculate monthly average delivery tasks completed per hour on all delivery platforms (using imputed hours when necessary, as described above) and separately for DoorDash since it reports hours information without requiring imputation. Individuals who do not appear in the data or have no observations of delivery activities for a given month are excluded from the corresponding analysis for that month.

The monthly utilization rate, average wait time per task, and average distance between tasks can be calculated for Uber Eats and Grubhub, because these sources consistently report request and end time and census-block-level location information for individual trips.[45] The monthly utilization rate is defined as the monthly total revenue-generating hours (request time to end time) divided by monthly total hours worked. Individuals who do not appear in the data for Uber Eats and/or Grubhub in a given month are excluded from the utilization rate analysis for that month.

The average wait time per task is the monthly average gap between the request time of a trip and the end time of the preceding trip. The monthly average distance between tasks is the average straight line distance between the centroid of the census block where a trip

[44] We choose the one-hour threshold to match the Seattle ordinance, which defines "available time" as a period of less than one hour between two periods of "engaged" or revenue-generating time. See https://www.seattle.gov/documents/Departments/LaborStandards/ABWMP_AffirmativeRecordsReportingGuide_04172025.pdf (accessed September 24, 2025).

[45] We omit Instacart because gig work on that platform involves both shopping and delivery, so is qualitatively different from pure delivery platforms.

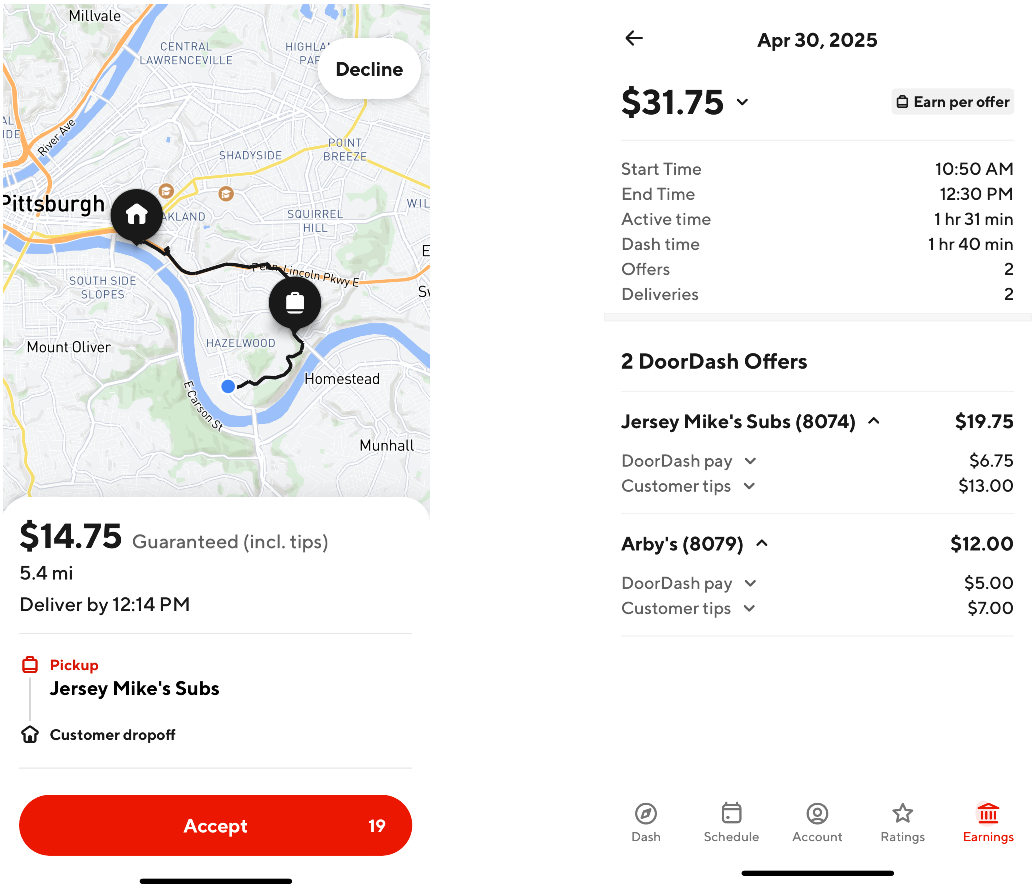
starts and where the preceding trip ends. To construct the two variables, we incorporate the task-level dataset into our main activity-level data, allowing us to observe location and timing information for each task within batched observations. We treat wait times and distances between tasks within a batch as zeros. Centroids for census blocks are constructed using the U.S. Census Bureau's census-block-level TIGER/Line shapefile. Individuals who do not appear in the data or do not have more than one observation for Uber Eats or Grubhub in a given month are excluded from the analyses of average wait time per task and average distance between tasks for that month.

Monthly rideshare tasks and earnings. For each worker-month, we construct an indicator for performing any rideshare tasks, the number of rideshare tasks completed, total rideshare earnings (the sum of base, tip, bonus, incentive, and adjustment earnings). Individuals who do not appear in the data for a given month are coded as not performing any rideshare tasks, completing zero rideshare tasks, and earning zero dollars from rideshare work in that month.

B Appendix Figures

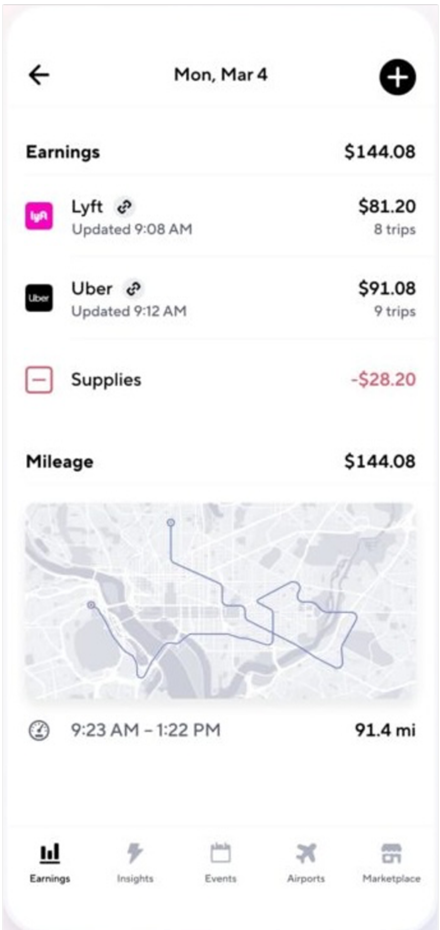
Figure B.1: Example Delivery Offer, DoorDash

(A) Information Shown Upon Offer (B) Information Shown Upon Completion



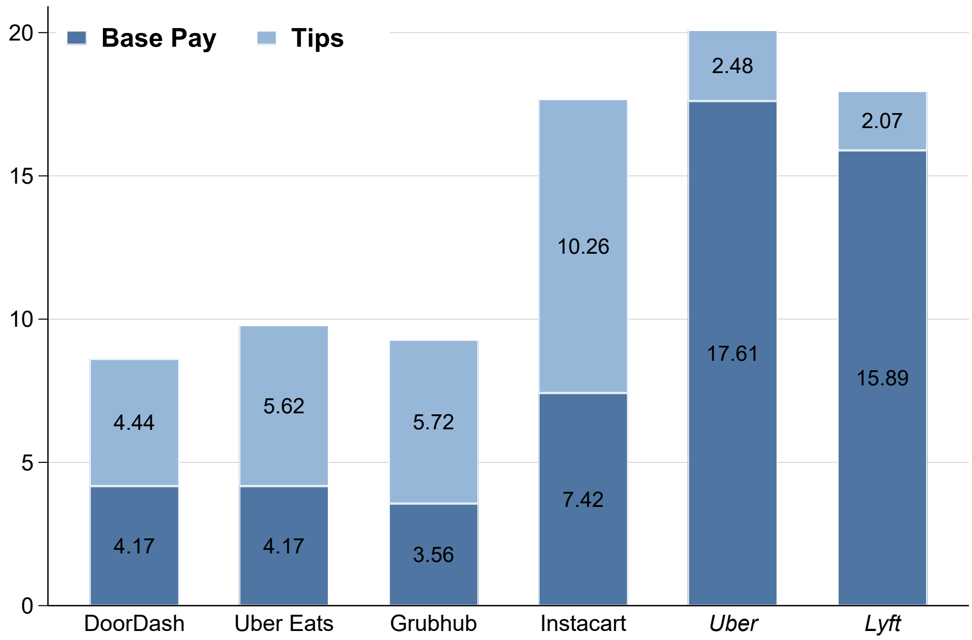
Notes: Panel (A) displays an offered delivery task on DoorDash in Pittsburgh (where no minimum pay standard is in effect). The driver is at the location denoted by the blue circle, the pick-up point is denoted by a black circle with an icon of a white bag, and the drop-off location is denoted by a black circle with an icon of a white house. Panel (B) displays the session summary after the completion of the offered task and one prior task. Information about base pay and tips is provided only at the end of the session. Note that, in this example, the guaranteed $14.75 pay included $7 in tips made at checkout, but the customer added an additional $5 tip after drop-off, likely due to the excellent service provided by the three academic economists handing off their food. Similar information is provided to drivers before and after tasks are completed on Uber Eats.

Figure B.2: Gridwise App Screenshot



Notes: This figure shows an example screeshot of Gridwise app. Screenshot recorded in the second half of 2024.

Figure B.3: Average Pay Per Task, Washington State (August 2023–December 2023)

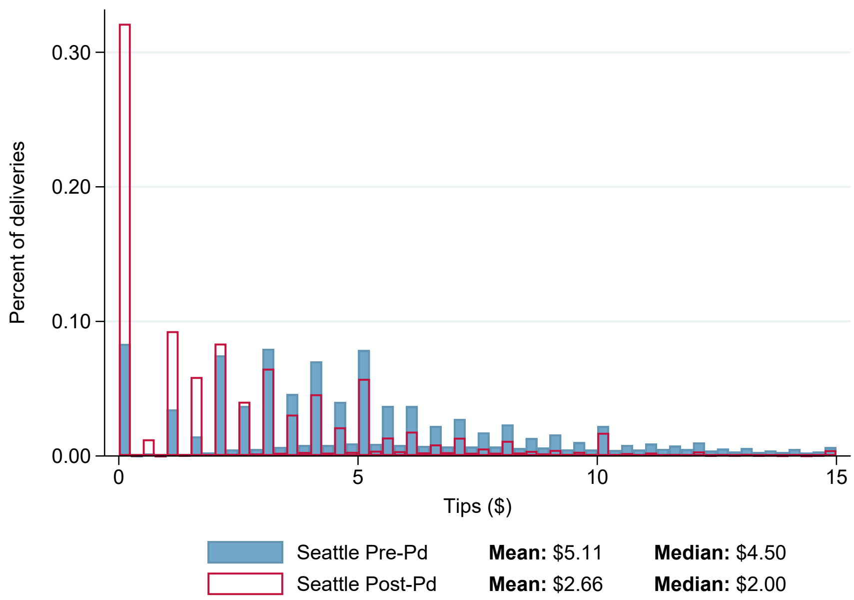


Notes: This figure calculates average base pay per task and average tips per task for tasks completed in Washington State from August 2023 to December 2023 on delivery platforms DoorDash, Uber Eats, Grubhub, and Instacart and rideshare platforms Uber and Lyft. Numbers presented on top of the bars report average base pay and average tips per task on corresponding platforms.

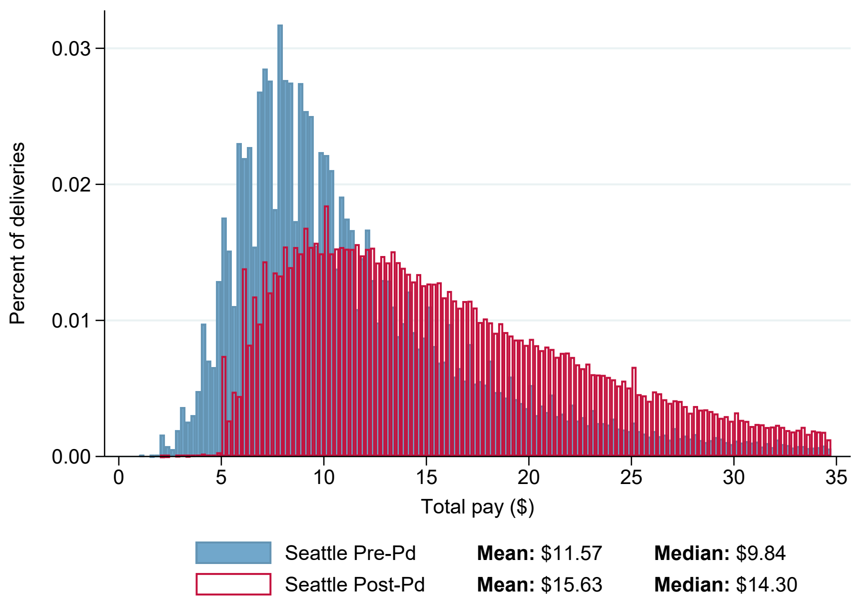
Source: Authors' tabulations using Gridwise data.

Figure B.4: Distributions of Pay across Deliveries in Seattle, by Period

(A) Tip Pay



(B) Total Pay

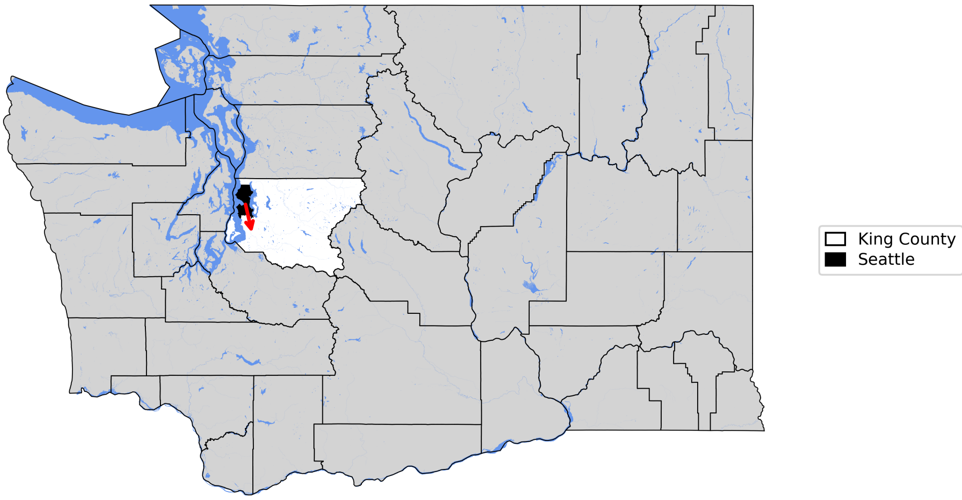


Notes: Panel (A) presents the distributions of tip pay across deliveries completed in Seattle in the pre-ordinance period (blue solid bars) and post-ordinance period (red hollow bars). Panel (B) presents the corresponding plot for total pay, where total pay includes base pay, tip pay, and bonus pay. Each bin represents $0.25. The figures omit observations above the 95th percentile of tip pay or total pay across deliveries completed in Seattle for ease of visualization. Text on the right-hand side of the legends presents means and medians of tip pay or total pay across deliveries completed in the pre-ordinance period and post-ordinance period in the sample.

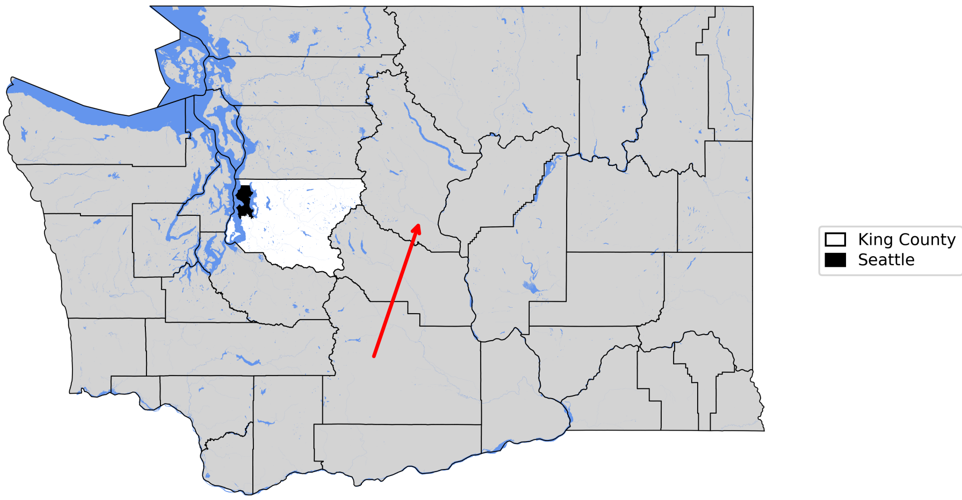
Source: Authors' analysis of Gridwise data.

Figure B.5: Examples of Activities by Geographic Group (Seattle/non-Seattle/Residual)

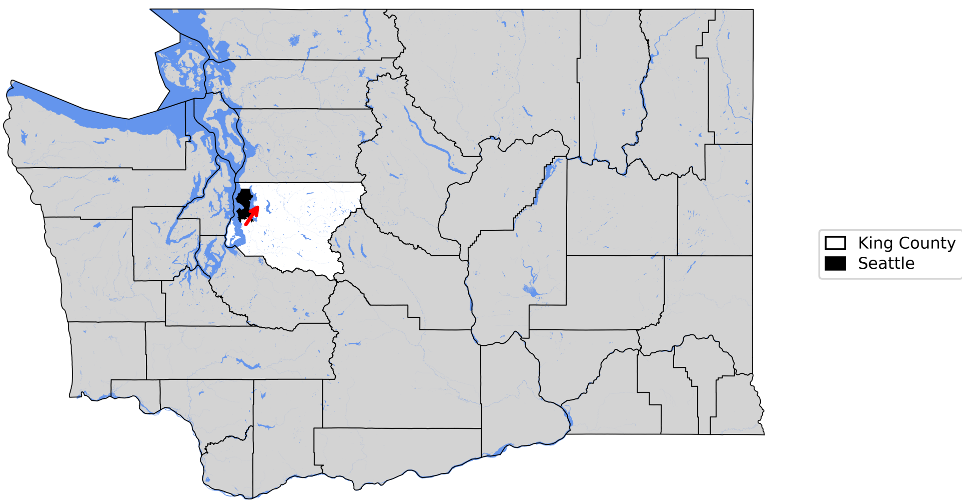
(A) Example of Seattle Activity



(B) Example of non-Seattle Activity



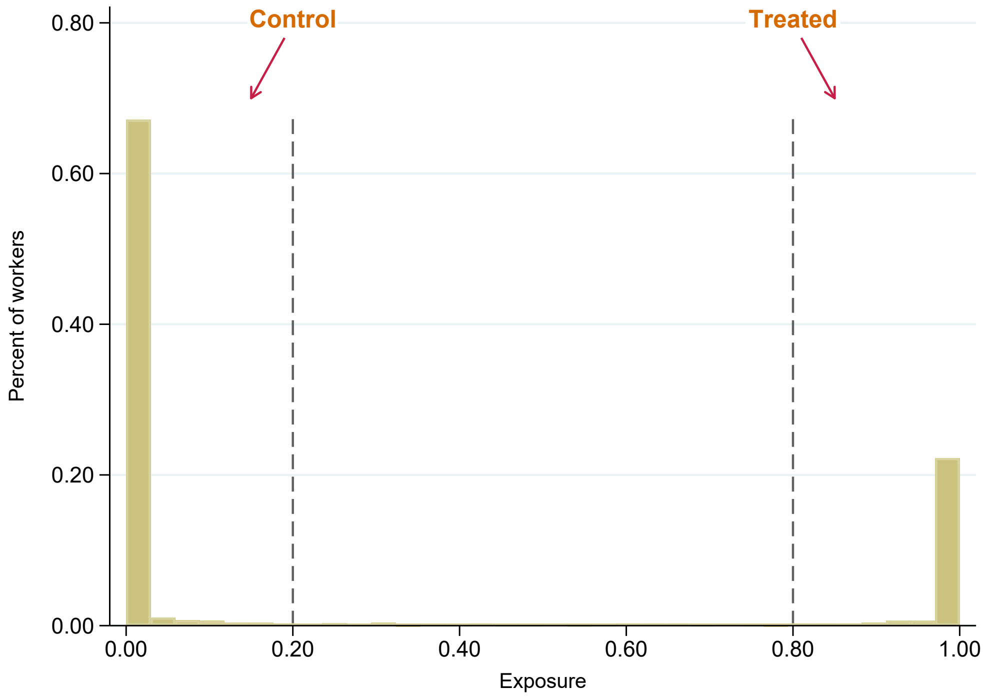
(C) Example of Residual Activity



Notes: This figure presents examples of delivery activities classified into the three geographic groups—Seattle group, non-Seattle group, and residual group—based on the geographic regions where activities start and end. The black region is the city of Seattle. The white region is King County outside of Seattle. The gray region is Washington State outside of King County. The Seattle group (Panel A) contains delivery activities that start or end in the city of Seattle. The non-Seattle group (Panel B) contains delivery activities that start and end outside of King County (which contains Seattle). The residual group (Panel C) contains delivery activities that start (end) in other parts of King County outside Seattle and end (start) not in the city of Seattle and activities with missing start or end geographic region information.

Source: Authors' map creation using the U.S. Census Bureau's TIGER/Line shapefile data.

Figure B.6: Distribution of Exposure Measure across Workers



Notes: This figure presents the distribution of our measure of exposure to Seattle across workers in our sample. Exposure is calculated as the share of delivery earnings a worker made from performing Seattle tasks prior to the Seattle minimum pay ordinance's implementation. For a detailed description of the calculation, see Section 3.2.

Source: Authors' analysis of Gridwise data.

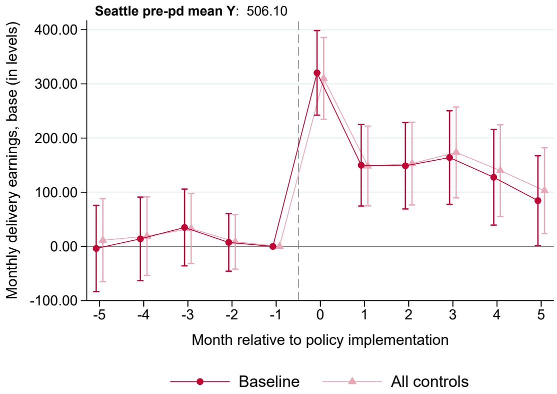
Figure B.7: Treatment and Control Group Mean Delivery Outcomes, More-Attached Workers

Notes: Plots show treatment and control group means for the specified outcomes (in levels) in each month relative to ordinance implementation. Series in darker colors with solid triangle markers and solid lines are means for the treatment group. Series in lighter colors with hollow triangle markers and dashed lines are means for the control group. Month 0 denotes the first event month following ordinance implementation, and month −1 denotes the event month before the ordinance. Individuals who do not appear in the data for a given month are coded as completing zero delivery tasks, working zero hours for delivery work, and earning zero dollars from delivery work in that month; "per-task" pay measures (outcomes in Panels G–I) are coded as missing for those individuals in those months. All samples are restricted to more-attached workers, defined as workers who performed delivery tasks above the median in the pre-policy period.

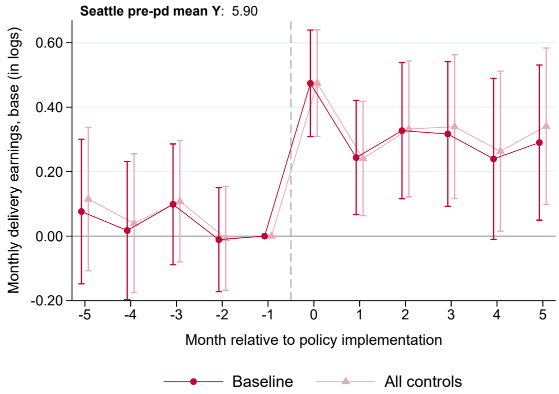
Figure B.8: Effects on Delivery Earnings – More-Attached Workers

(A) Base Earnings

(I) Levels

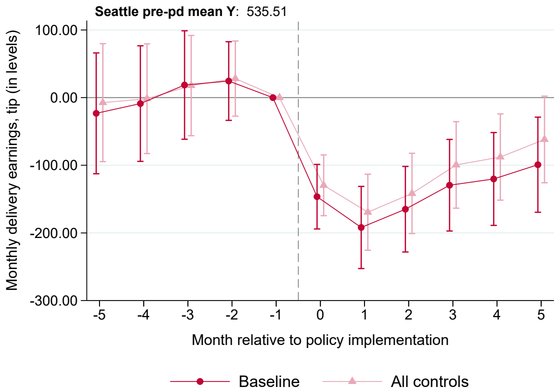


(II) Logs

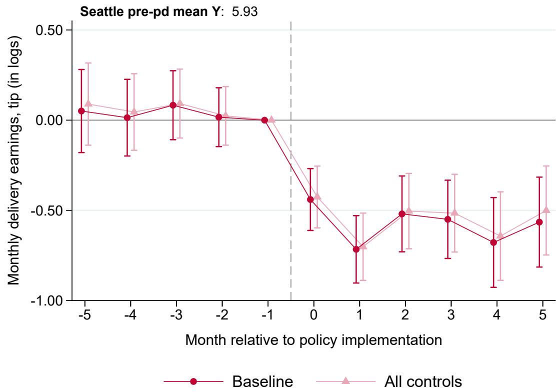


(B) Tip Earnings

(I) Levels

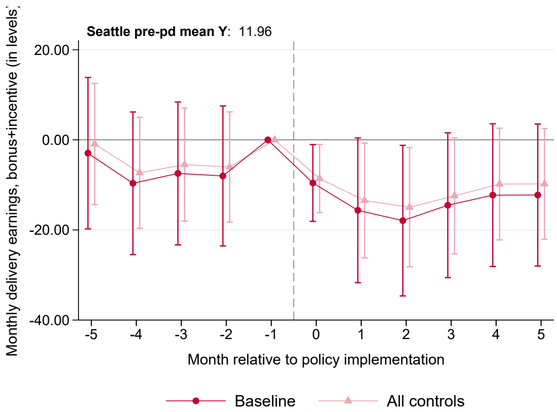


(II) Logs

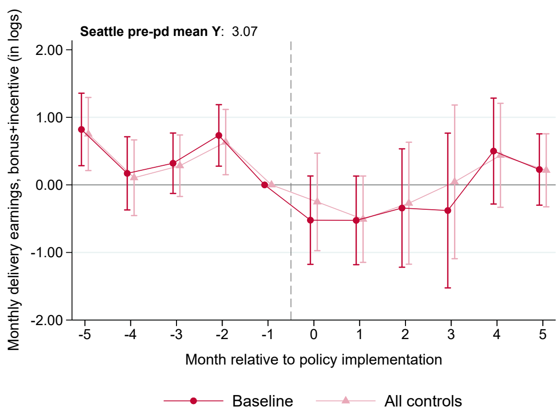


(C) Bonus & Incentive Earnings

(I) Levels

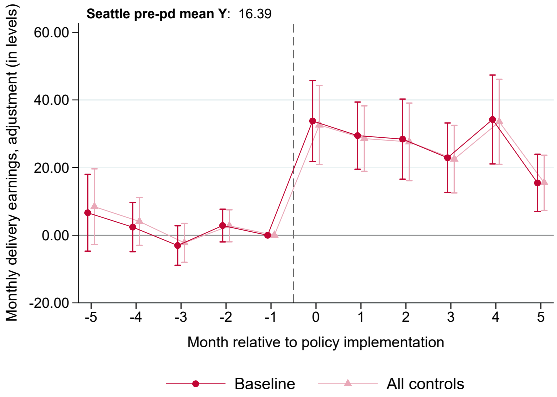


(II) Logs

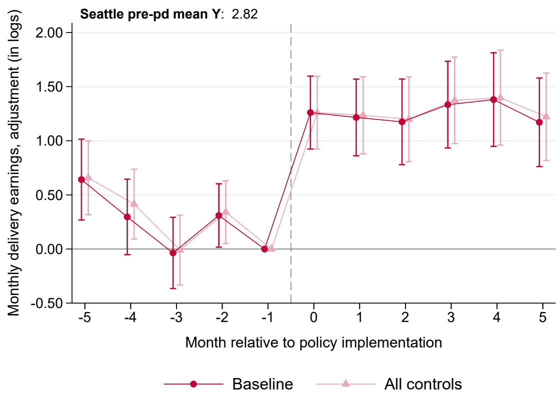


(D) Adjustment Earnings

(I) Levels

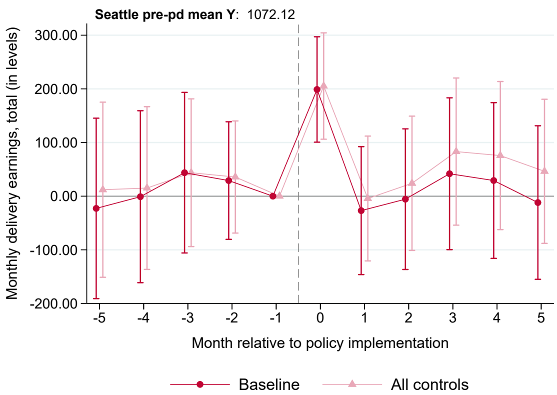


(II) Logs

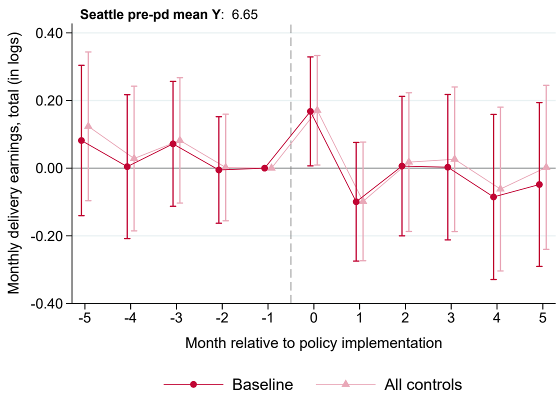


(E) Total Earnings

(I) Levels



(II) Logs

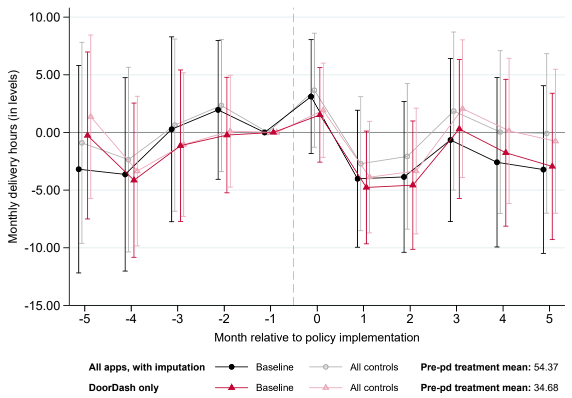


Notes: The figures plot estimates of β_k from Equation (1) for the outcomes listed in the panel titles for the sample of more-attached workers. Note that in Panel (E), total earnings include base earnings, tip earnings, bonus earnings, incentive earnings, and adjustment earnings. For each outcome, Panel (I) examines the outcome in levels, inclusive of zero values, and Panel (II) examines the outcome in logs in which zero values in levels are coded as missing in that month. Month 0 denotes the first event month following ordinance implementation, and month -1 denotes the event month before the ordinance. Worker fixed effects and event year-month fixed effects are included in the estimation. Plots in darker colors with circle markers are estimates without including additional controls (Baseline), while plots in lighter colors with triangle markers include the full set of additional controls (All controls)—a set of pre-policy individual worker covariates interacted with indicators for event months. Covariates consist of delivery (rideshare) tasks, months active in delivery (rideshare) work, share of earnings from rideshare versus delivery work, and log total earnings, all measured during the pre-policy period. All samples are restricted to more-attached workers, defined as workers who performed delivery tasks above the median in the pre-policy period. The whiskers represent 95 percent confidence intervals. Standard errors are clustered at the worker level. Text in the top left-hand corner in each panel presents the mean of the corresponding outcome prior to the ordinance for exposed workers.

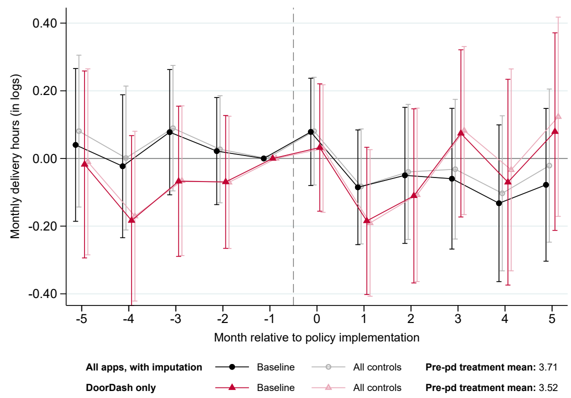
Figure B.9: Effects on Delivery Hours and Active Delivery Days – More-Attached Workers

(A) Hours

(I) Levels

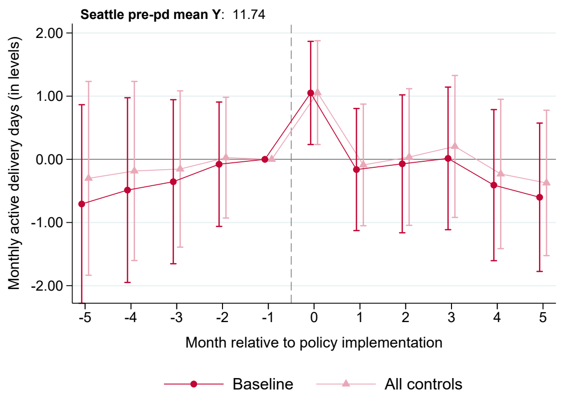


(II) Logs

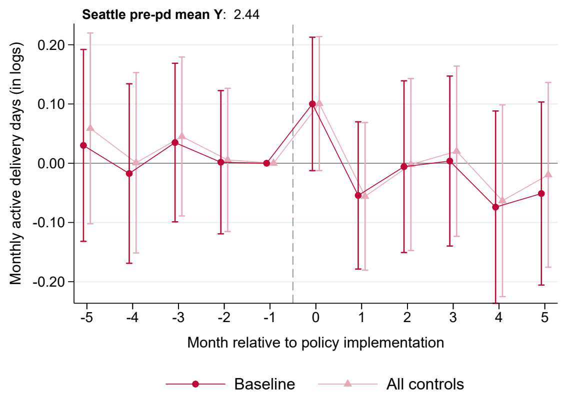


(B) Active Days

(I) Levels



(II) Logs

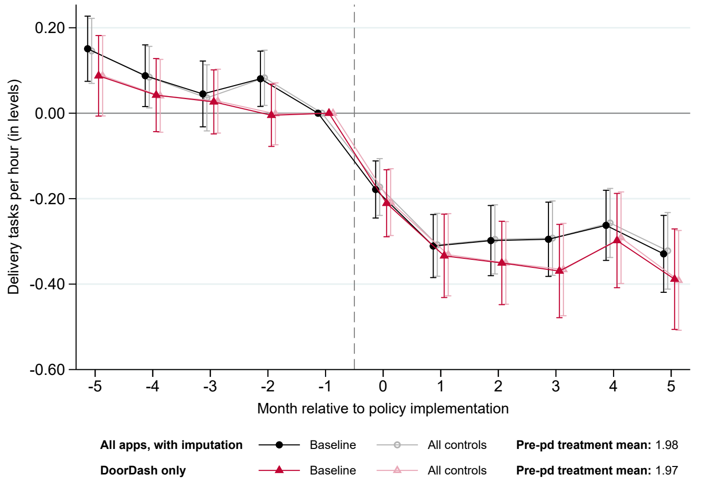


Notes: The figures plot estimates of β_k from Equation (1) using the sample of more-attached workers for monthly delivery hours (Panel A) and monthly active days in delivery (Panel B). In Panel (A), plots in black with circle markers use imputed monthly total hours for all delivery platforms, while plots in red with triangle markers use observed hours for DoorDash. In both cases, Panel (I) examines the outcome in levels, inclusive of zero values, and Panel (II) examines the outcome in logs, in which zero values in levels are coded as missing in that month. Month 0 denotes the first event month following ordinance implementation, and month -1 denotes the event month before the ordinance. Worker fixed effects and event year-month fixed effects are included in the estimation. Plots in darker colors are estimates without including additional controls (Baseline), while plots in lighter colors include the full set of additional controls (All controls)—a set of pre-policy individual worker covariates interacted with indicators for event months. Covariates consist of delivery (rideshare) tasks, months active in delivery (rideshare) work, share of earnings from rideshare versus delivery work, and log total earnings, all measured during the pre-policy period. All samples are restricted to more-attached workers, defined as workers who performed delivery tasks above the median in the pre-policy period. The whiskers represent 95 percent confidence intervals. Standard errors are clustered at the worker level. Text on the right-hand side of the legend in each panel of Panel (A) presents means of corresponding outcomes prior to the ordinance for exposed workers. Text in the top left-hand corner in each panel of Panel (B) presents the mean of the corresponding outcome prior to the ordinance for exposed workers.

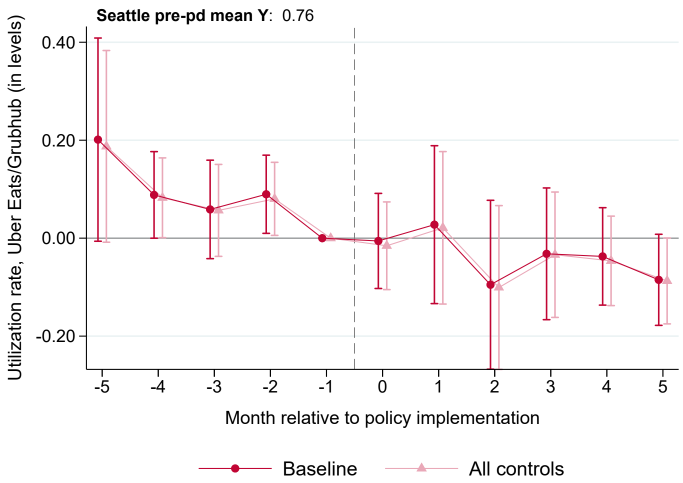
Source: Authors' analysis of Gridwise data.

Figure B.10: Effects on Delivery Time Use – More-Attached Workers

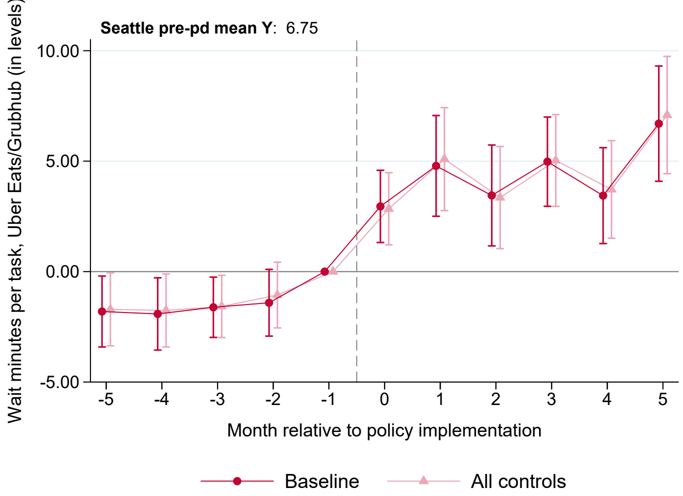
(A) Tasks Per Hour



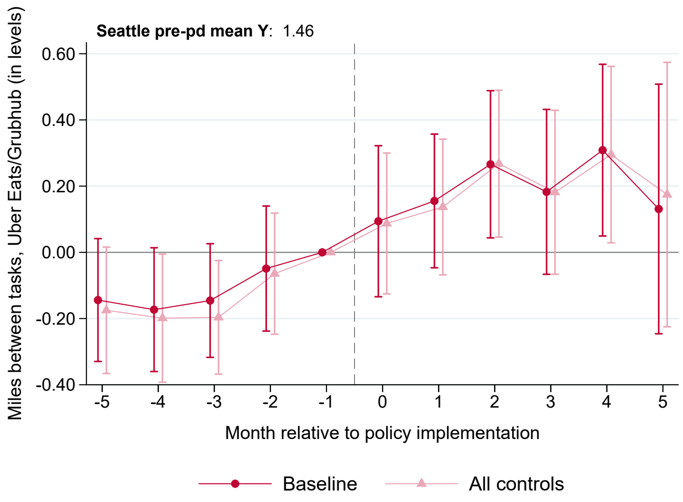
(B) Utilization Rate



(C) Task Wait Time (in Minutes)



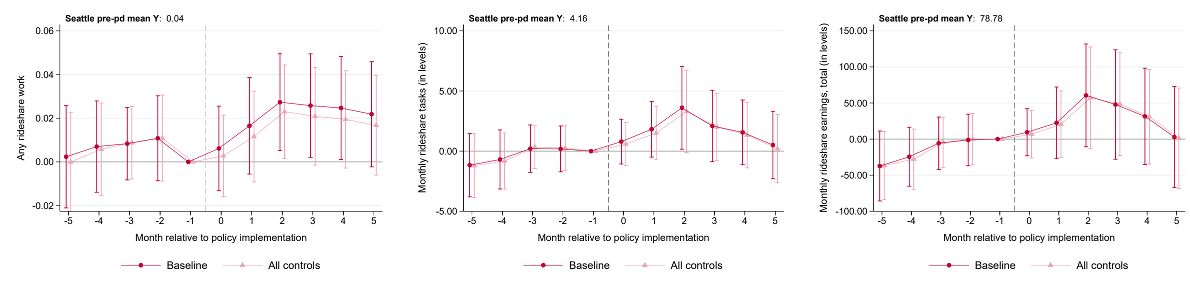
(D) Distance Between Tasks (in Miles)



Notes: The figures plot estimates of β_k from Equation (1) for the outcomes listed in the panel titles for the sample of more-attached workers. In Panel (A), plots in black with circle markers use imputed monthly total hours to calculate tasks per hour for all delivery platforms, while plots in red with triangle markers use observed tasks per hour for DoorDash. Given data availability, utilization rates in Panel (B), task wait times in Panel (C), and distances between tasks in Panel (D) are calculated for Uber Eats and Grubhub. All outcomes are calculated in levels. Outcomes corresponding to zero tasks completed on the relevant delivery platform(s) in a given event month are coded as missing in that month. Month 0 denotes the first event month following ordinance implementation, and month -1 denotes the event month before the ordinance. Worker fixed effects and event year-month fixed effects are included in the estimation. Plots in darker colors are estimates without including additional controls (Baseline), while plots in lighter colors include the full set of additional controls (All controls)—a set of pre-policy individual worker covariates interacted with indicators for event months. Covariates consist of delivery (rideshare) tasks, months active in delivery (rideshare) work, share of earnings from rideshare versus delivery work, and log total earnings, all measured during the pre-policy period. All samples are restricted to more-attached workers, defined as workers who performed delivery tasks above the median in the pre-policy period. The whiskers represent 95 percent confidence intervals. Standard errors are clustered at the worker level. Text on the right-hand side of the legend in Panel (A) presents means of corresponding outcomes prior to the ordinance for exposed workers. Text in the top left-hand corner in Panels (B), (C), and (D) presents the mean of the corresponding outcome prior to the ordinance for exposed workers.

Figure B.11: Effects on Rideshare Work and Earnings

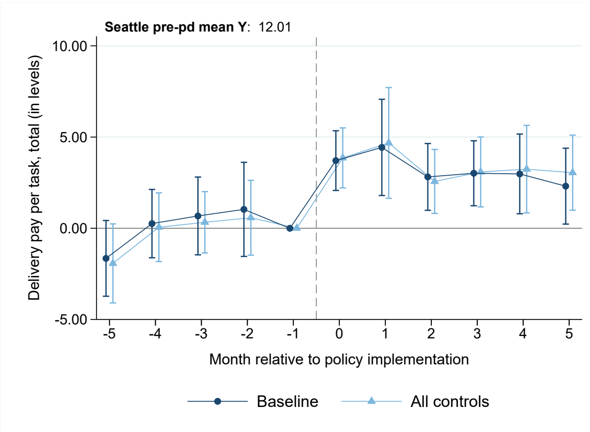
(A) Any Rideshare Work (B) Rideshare Tasks (C) Rideshare Earnings



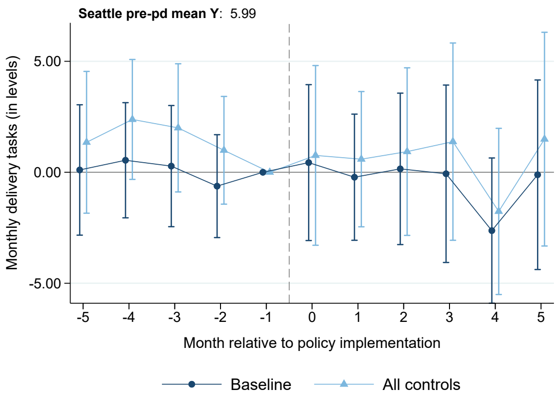
Notes: The figures plot, for the sample of more-attached workers, estimates of β_k from Equation (1) for an indicator for performing any rideshare tasks per month (Panel A), the number of monthly completed rideshare tasks (Panel B), and monthly rideshare total earnings (Panel C), where total earnings include base earnings, tip earnings, bonus earnings, incentive earnings, and adjustment earnings. All outcomes are calculated in levels, inclusive of zero values. Month 0 denotes the first event month following ordinance implementation, and month -1 denotes the event month before the ordinance. Worker fixed effects and event year-month fixed effects are included in the estimation. Plots in darker colors with circle markers are estimates without including additional controls (Baseline), while plots in lighter colors with triangle markers include the full set of additional controls (All controls)—a set of pre-policy individual worker covariates interacted with indicators for event months. Covariates consist of delivery (rideshare) tasks, months active in delivery (rideshare) work, share of earnings from rideshare versus delivery work, and log total earnings, all measured during the pre-policy period. All samples are restricted to more-attached workers, defined as workers who performed delivery tasks above the median in the pre-policy period. The whiskers represent 95 percent confidence intervals. Standard errors are clustered at the worker level. Text in the top left-hand corner in each panel presents the mean of the corresponding outcome prior to the ordinance for exposed workers.

Figure B.12: Effects on Delivery Pay Per Task, Tasks, and Earnings – Less-Attached Workers

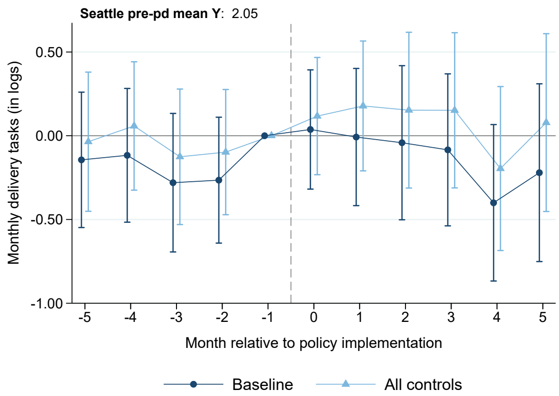
(A) Total Delivery Pay Per Task



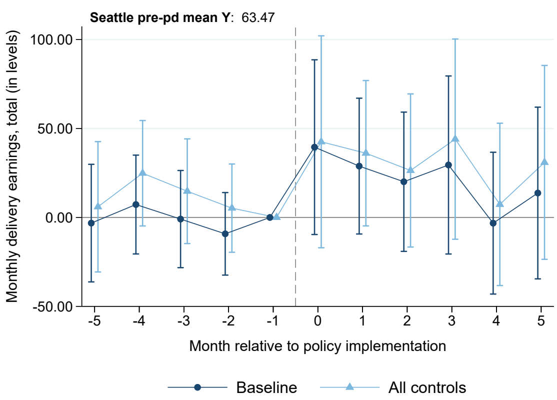
(B) # Delivery Tasks Including Zeros



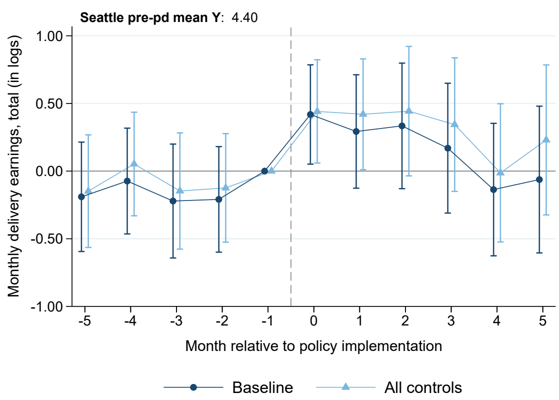
(C) Log(Delivery Tasks) if Positive



(D) Total Delivery Earnings Including Zeros



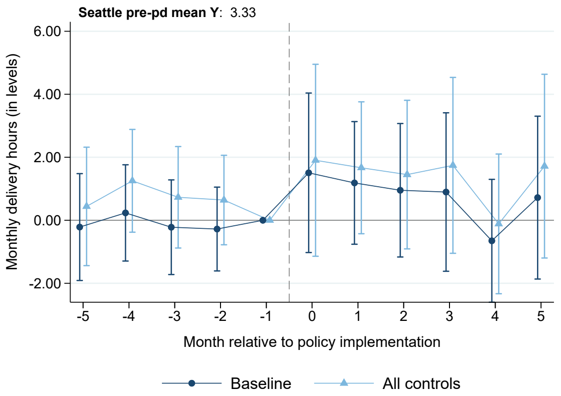
(E) Log(Total Delivery Earnings) if Positive



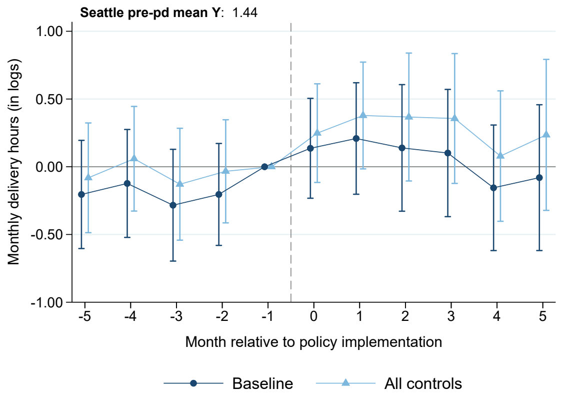
Notes: Each panel plots estimates of β_k from Equation (1) for the outcome listed in the panel title using the sample of less-attached workers. Note that total pay includes base pay, tip pay, bonus pay, incentive pay, and adjustment pay. Outcomes in levels (Panels B and D) are inclusive of zero values, while those in logs (Panels C and E) code zero values in levels in a given event month as missing in that month. Month 0 denotes the first event month following ordinance implementation, and month -1 denotes the event month before the ordinance. Worker fixed effects and event year-month fixed effects are included in the estimation. Plots in darker colors with circle markers are estimates without including additional controls (Baseline), while plots in lighter colors with triangle markers include the full set of additional controls (All controls)—a set of pre-policy individual worker covariates interacted with indicators for event months. Covariates consist of delivery (rideshare) tasks, months active in delivery (rideshare) work, share of earnings from rideshare versus delivery work, and log total earnings, all measured during the pre-policy period. All samples are restricted to less-attached workers, defined as workers who performed delivery tasks below the median in the pre-policy period. The whiskers represent 95 percent confidence intervals. Standard errors are clustered at the worker level. Text in the top left-hand corner in each panel presents the mean of the corresponding outcome prior to the ordinance for exposed workers.

Figure B.13: Effects on Delivery Hours and Active Delivery Days – Less-Attached Workers

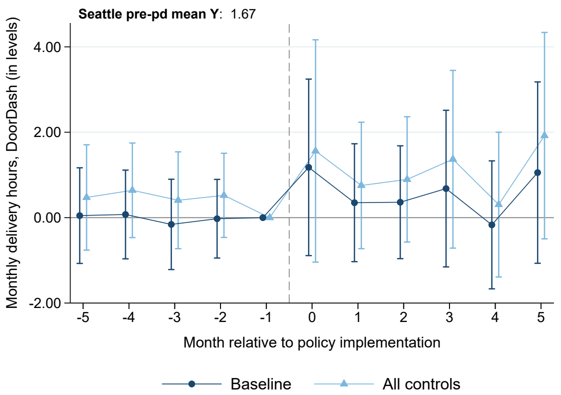
(A) Delivery Hours Including Zeros,
All Apps with Imputation



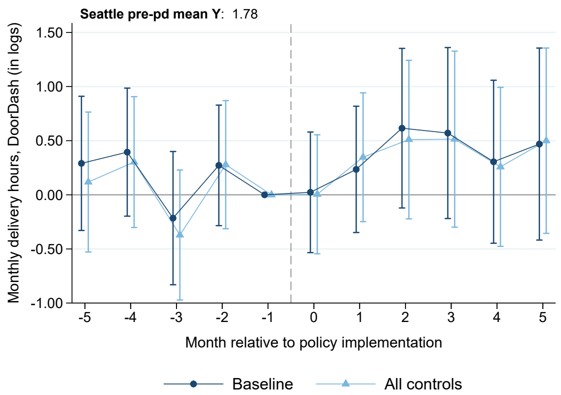
(B) Log(Delivery Hours) if Positive,
All Apps with Imputation



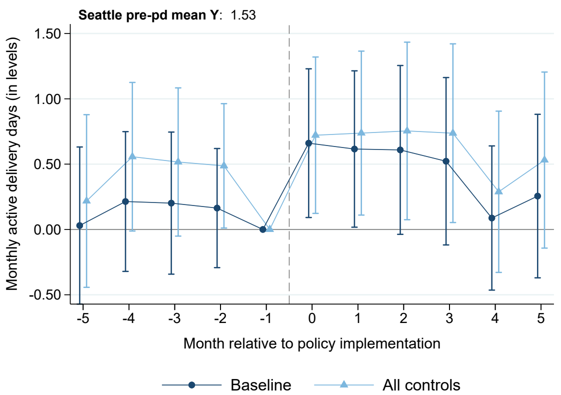
(C) Delivery Hours Including Zeros,
DoorDash Only



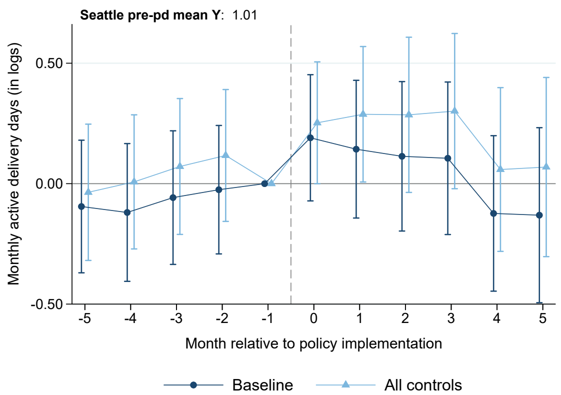
(D) Log(Delivery Hours) if Positive,
DoorDash Only



(E) Active Delivery Days Including Zeros



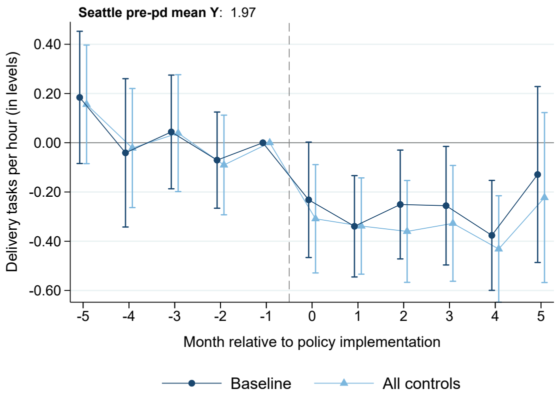
(F) Log(Active Delivery Days) if Positive



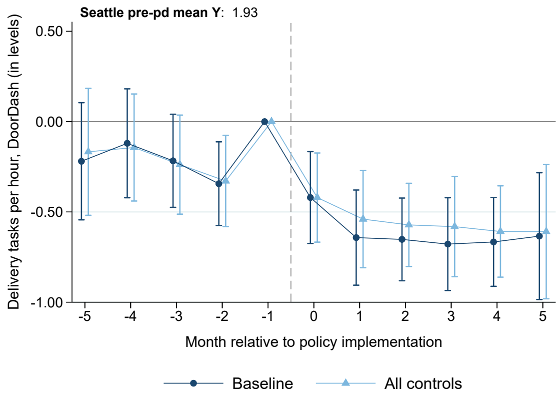
Notes: Each panel plots estimates of β_k from Equation (1) for the outcome listed in the panel title using the sample of less-attached workers. Panels (A) and (B) plot effects on total monthly hours for all delivery platforms including imputed values, while Panels (C) and (D) restrict attention to observed monthly hours on DoorDash only. Panels (E) and (F) show estimates for the number of monthly active days on all delivery platforms. Outcomes in levels (Panels A, C, and E) are inclusive of zero values, while those in logs (Panels B, D, and F) code zero values in levels in a given event month as missing in that month. Month 0 denotes the first event month following ordinance implementation, and month -1 denotes the event month before the ordinance. Worker fixed effects and event year-month fixed effects are included in the estimation. Plots in darker colors with circle markers are estimates without including additional controls (Baseline), while plots in lighter colors with triangle markers include the full set of additional controls (All controls)—a set of pre-policy individual worker covariates interacted with indicators for event months. Covariates consist of delivery (rideshare) tasks, months active in delivery (rideshare) work, share of earnings from rideshare versus delivery work, and log total earnings, all measured during the pre-policy period. All samples are restricted to less-attached workers, defined as workers who performed delivery tasks below the median in the pre-policy period. The whiskers represent 95 percent confidence intervals. Standard errors are clustered at the worker level. Text in the top left-hand corner in each panel presents the mean of the corresponding outcome prior to the ordinance for exposed workers.

Figure B.14: Effects on Delivery Time Use – Less-Attached Workers

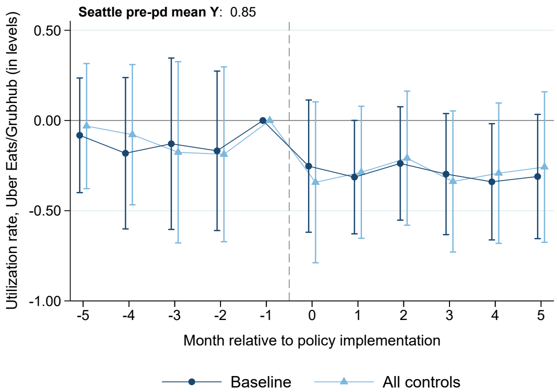
(A) Tasks Per Hour, All Apps with Imputation



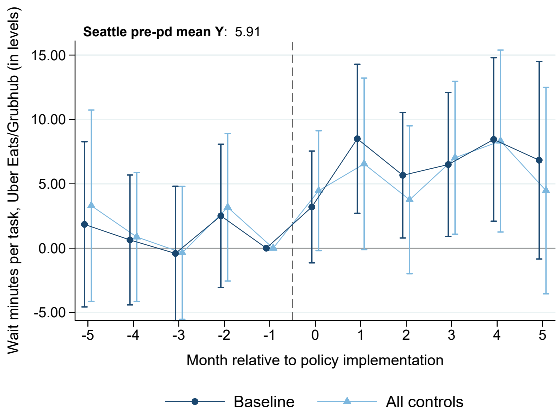
(B) Tasks Per Hour, DoorDash Only



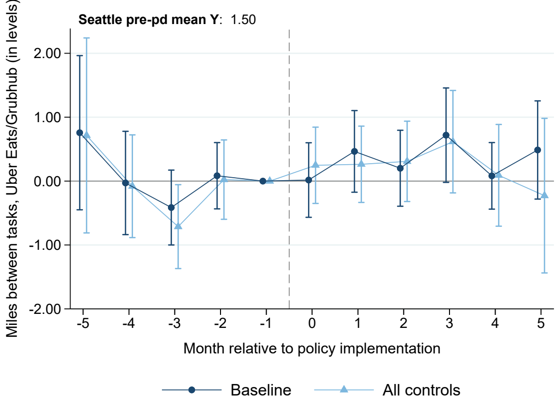
(C) Utilization Rate



(D) Task Wait Time (in Minutes)



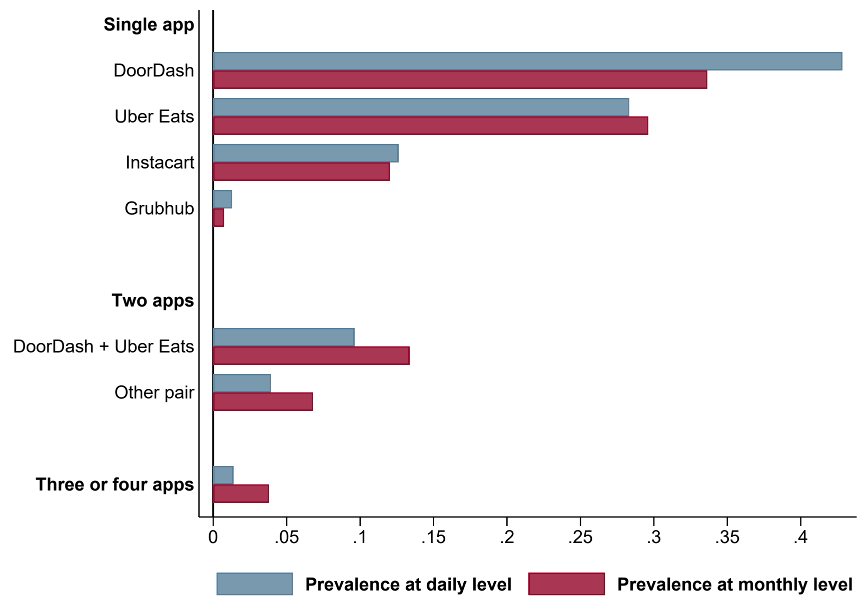
(E) Distance Between Tasks (in Miles)



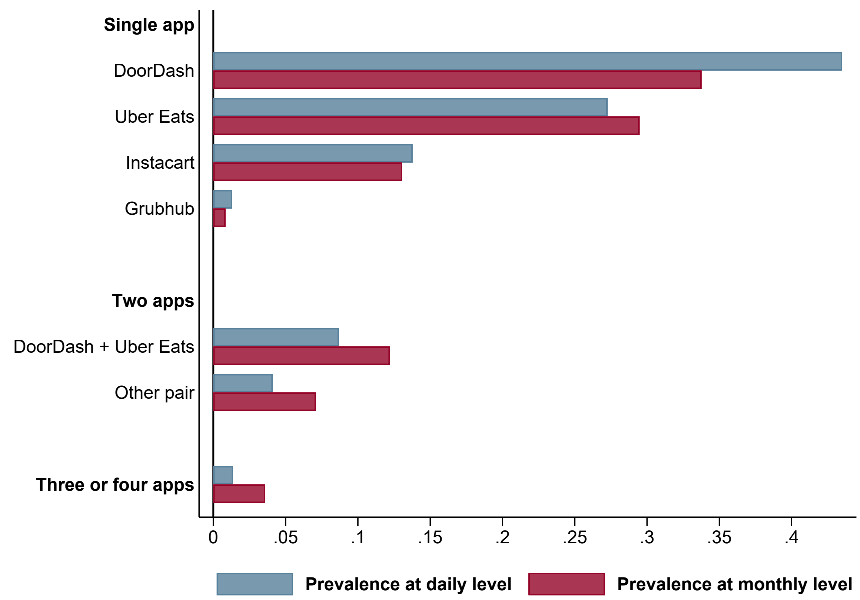
Notes: Each panel plots estimates of β_k from Equation (1) for the outcome listed in the panel title using the sample of less-attached workers. Panel (A) shows tasks per hour, calculated for all delivery platforms using imputed hours when necessary. Panel (B) shows tasks per hour using observed hours information for DoorDash alone. Given data availability, the utilization rate in Panel (C), task wait time in Panel (D), and distance between tasks in Panel (E) are calculated for Uber Eats and Grubhub. All outcomes are calculated in levels, and observations with zero tasks completed on the relevant delivery platform(s) in a given event month are coded as missing in that month. Month 0 denotes the first event month following ordinance implementation, and month -1 denotes the event month before the ordinance. Worker fixed effects and event year-month fixed effects are included in the estimation. Plots in darker colors with circle markers are estimates without including additional controls (Baseline), while plots in lighter colors with triangle markers include the full set of additional controls (All controls)—a set of pre-policy individual worker covariates interacted with indicators for event months. Covariates consist of delivery (rideshare) tasks, months active in delivery (rideshare) work, share of earnings from rideshare versus delivery work, and log total earnings, all measured during the pre-policy period. All samples are restricted to less-attached workers, defined as workers who performed delivery tasks below the median in the pre-policy period. The whiskers represent 95 percent confidence intervals. Standard errors are clustered at the worker level. Text in the top left-hand corner in each panel presents the mean of the corresponding outcome prior to the ordinance for exposed workers.

Figure B.15: Distribution of Delivery App Use

(A) Full Sample



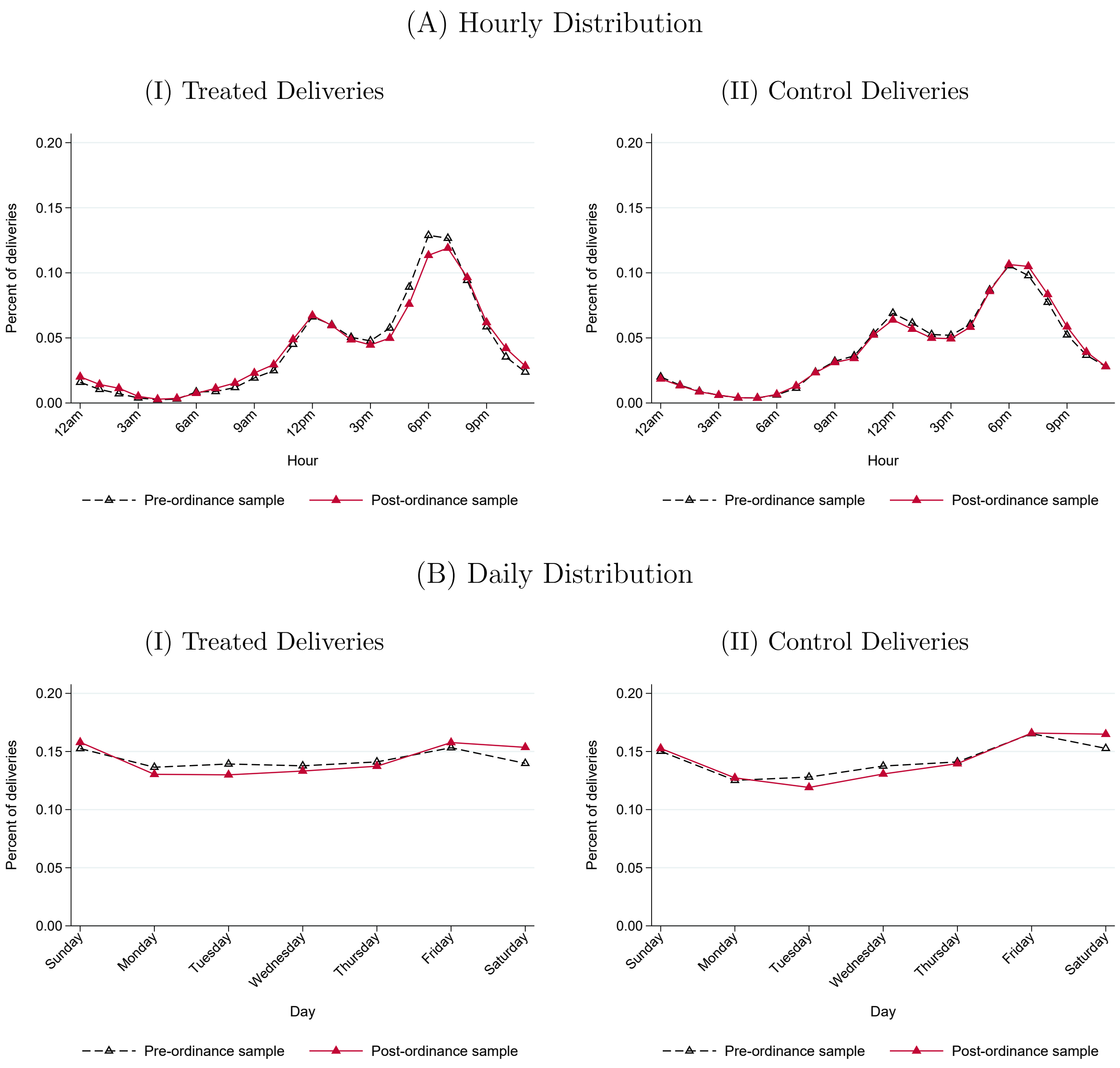
(B) Pre-Ordinance Sample



Notes: Panel (A) presents the share of days (blue bars) and months (red bars) worked in which drivers in the full Gridwise sample are either active only on a single delivery app (broken down by whether that app is DoorDash, Uber Eats, Instacart, or Grubhub), active on two delivery apps (broken down by DoorDash+Uber Eats and then pooling all other pairs), or active on three or four delivery apps. Panel (B) presents the corresponding plot restricting to the pre-ordinance period.

Source: Authors' analysis of Gridwise data.

Figure B.16: Time Distribution of Deliveries



Notes: Panels (A) and (B) plot, respectively, the distributions of hours of the day and days of the week across deliveries completed in the pre-ordinance period and post-ordinance period. Plots in black are for the pre-ordinance period and those in red are for the post-ordinance period. In each panel, Panel (I) examines the treatment group, and Panel (II) examines the control group. All samples are restricted to deliveries completed at Uber Eats and Grubhub, given data availability.

Source: Authors' analysis of Gridwise data.

C Appendix Tables

Table C.1: Effects on Geographic Distribution of Delivery Tasks

	Seattle		Buffer area		Outside King County	
	(1)	(2)	(3)	(4)	(5)	(6)
A. More-attached workers						
Treated × Post	-0.051***	-0.052***	0.050**	0.052**	0.001	0.000
	(0.014)	(0.014)	(0.018)	(0.019)	(0.013)	(0.014)
N worker-months	9,906	9,906	9,906	9,906	9,906	9,906
N workers	1,265	1,265	1,265	1,265	1,265	1,265
Treatment pre-pd mean Y	0.495	0.495	0.496	0.496	0.009	0.009
Treatment post-pd mean Y	0.423	0.423	0.534	0.534	0.043	0.043
B. Less-attached workers						
Treated × Post	-0.122***	-0.116***	0.062*	0.068*	0.059**	0.049*
	(0.033)	(0.034)	(0.032)	(0.033)	(0.021)	(0.020)
N worker-months	4,160	4,160	4,160	4,160	4,160	4,160
N workers	941	941	941	941	941	941
Treatment pre-pd mean Y	0.602	0.602	0.389	0.389	0.009	0.009
Treatment post-pd mean Y	0.461	0.461	0.473	0.473	0.066	0.066
Controls		✓		✓		✓

Notes: This table presents estimates of β from Equation (1) replacing the indicators for event months interacted with the term Treat_i with a single indicator for the post-ordinance period. The outcomes are the monthly shares of delivery tasks completed in each of the three geographic areas: Seattle (columns 1-2), buffer area—King County outside Seattle (columns 3-4), and area outside King County (columns 5-6). "Seattle" tasks start or end in the city of Seattle. "Buffer area" tasks start (end) in King County outside Seattle and do not end (start) in city of Seattle, including cases where the end (start) location is missing. "Outside King County" tasks both start and end outside of King County. In months during which a worker completes zero delivery tasks, all three shares are coded as missing. Worker fixed effects and event year-month fixed effects are included in the estimation. Columns 1, 3, and 5 present estimates without including additional controls, and columns 2, 4, and 6 present estimates including the full set of additional controls—a set of pre-policy individual worker covariates interacted with the indicator for the post-ordinance period. Covariates consist of delivery (rideshare) tasks, months active in delivery (rideshare) work, share of earnings from rideshare versus delivery work, and log total earnings, all measured during the pre-policy period. Panels (A) and (B) present estimates for the samples of more-attached and less-attached workers, respectively, defined as workers who performed delivery tasks above and below the median in the pre-policy period. Standard errors in parentheses are clustered at the worker level. The "treatment pre-period mean Y" is the mean of the corresponding outcome prior to the ordinance for exposed workers, while the "treatment post-period mean Y" reports means after the ordinance for exposed workers. * $p < .05$, ** $p < .01$, *** $p < .001$.

Table C.2: Effects on Delivery Time Use, Additional Outcomes

	Task time (min)		Task distance (mi)		Batched task share	
	(1)	(2)	(3)	(4)	(5)	(6)
A. More-attached workers						
Treated × Post	-0.764*	-0.702*	-0.016	-0.002	-0.060***	-0.063***
	(0.333)	(0.338)	(0.057)	(0.058)	(0.011)	(0.011)
N worker-months	4,763	4,763	4,674	4,674	9,906	9,906
N workers	666	666	655	655	1,265	1,265
Treatment pre-pd mean Y	23.030	23.030	2.056	2.056	0.325	0.325
Treatment post-pd mean Y	21.639	21.639	2.132	2.132	0.266	0.266
B. Less-attached workers						
Treated × Post	-0.301	-0.220	-0.439	-0.084	-0.046	-0.048
	(0.879)	(0.936)	(0.275)	(0.257)	(0.027)	(0.026)
N worker-months	2,253	2,253	2,018	2,018	4,160	4,160
N workers	529	529	475	475	941	941
Treatment pre-pd mean Y	24.027	24.027	2.950	2.950	0.329	0.329
Treatment post-pd mean Y	21.453	21.453	2.176	2.176	0.247	0.247
Controls		✓		✓		✓

Notes: This table presents estimates of β from Equation (1) replacing the indicators for event months interacted with the term Treat_i with a single indicator for the post-ordinance period. The outcomes are the monthly average minutes worked per task (columns 1-2), monthly average miles traveled per task (columns 3-4), and the monthly share of tasks that are in batches (columns 5-6). Task times and task distances are calculated for Uber Eats and Grubhub, given data availability. All outcomes are calculated in levels. Outcomes corresponding to zero tasks completed on the relevant delivery platforms in a given event month are coded as missing in that month. Worker fixed effects and event year-month fixed effects are included in the estimation. Columns 1, 3, and 5 present estimates without including additional controls, and columns 2, 4, and 6 present estimates including the full set of additional controls—a set of pre-policy individual worker covariates interacted with the indicator for the post-ordinance period. Covariates consist of delivery (rideshare) tasks, months active in delivery (rideshare) work, share of earnings from rideshare versus delivery work, and log total earnings, all measured during the pre-policy period. Panels (A) and (B) present estimates for the samples of more-attached and less-attached workers, respectively, defined as workers who performed delivery tasks above and below the median in the pre-policy period. Standard errors in parentheses are clustered at the worker level. The "treatment pre-period mean Y" is the mean of the corresponding outcome prior to the ordinance for exposed workers, while the "treatment post-period mean Y" reports means after the ordinance for exposed workers. * $p < .05$, ** $p < .01$, *** $p < .001$.

Table C.3: Robustness – Effects on Delivery Pay Per Task, Alternative Imputation Methods

	Base pay per task		Tip pay per task		Bonus+incentive+adjustment pay per task		Total pay per task	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
A. More-attached workers								
Treated × Post	4.091***	4.050***	-1.448***	-1.439***	0.976***	0.969***	3.502***	3.461***
	(0.213)	(0.213)	(0.151)	(0.148)	(0.185)	(0.189)	(0.288)	(0.292)
N worker-months	9,906	9,906	9,906	9,906	9,906	9,906	9,906	9,906
N workers	1,265	1,265	1,265	1,265	1,265	1,265	1,265	1,265
Treatment pre-pd mean Y	4.877	4.877	5.158	5.158	0.361	0.361	10.335	10.335
Treatment post-pd mean Y	9.046	9.046	3.629	3.629	1.245	1.245	13.754	13.754
B. Less-attached workers								
Treated × Post	4.198***	4.468***	-1.560***	-1.553***	0.634	0.824	3.203***	3.688***
	(0.452)	(0.474)	(0.284)	(0.296)	(0.446)	(0.498)	(0.618)	(0.673)
N worker-months	4,160	4,160	4,160	4,160	4,160	4,160	4,160	4,160
N workers	941	941	941	941	941	941	941	941
Treatment pre-pd mean Y	6.445	6.445	4.854	4.854	0.537	0.537	11.750	11.750
Treatment post-pd mean Y	9.532	9.532	3.405	3.405	0.862	0.862	13.685	13.685
Controls		✓		✓		✓		✓

Notes: This table presents estimates of β from Equation (1) replacing the indicators for event months interacted with the term Treat_i with a single indicator for the post-ordinance period. The outcomes are delivery base pay per task (columns 1-2), delivery tip pay per task (columns 3-4), delivery bonus, incentive, and adjustment pay per task (columns 5-6), and delivery total pay per task (columns 7-8), where total pay includes base pay, tip pay, bonus pay, incentive pay, and adjustment pay, using alternative imputation methods. Tip pay is imputed by subtracting base pay and bonus pay from the recorded total pay in the raw data after imputing missing bonus pay values with zeros. Bonus pay is imputed by subtracting base pay and tip pay from the recorded total pay in the raw data after imputing missing tip pay values with zeros. Total pay documented in this table is imputed as the sum of base pay, tip pay, bonus pay, incentive pay, and adjustment pay after imputing missing tip and bonus pay values with zeros. For a detailed description of the imputation procedure, see Appendix Section A.2. All outcomes are calculated in levels. Outcomes corresponding to zero delivery tasks in a given event month are coded as missing in that month. Worker fixed effects and event month fixed effects are included in the estimation. Columns 1, 3, 5, and 7 present estimates without including additional controls, and columns 2, 4, 6, and 8 present estimates including the full set of additional controls—a set of pre-policy individual worker covariates interacted with the indicator for the post-ordinance period. Covariates consist of delivery (rideshare) tasks, months active in delivery (rideshare) work, share of earnings from rideshare versus delivery work, and log total earnings, all measured during the pre-policy period. Panels (A) and (B) present estimates for the samples of more-attached and less-attached workers, respectively, defined as workers who performed delivery tasks above and below the median in the pre-policy period. Standard errors in parentheses are clustered at the worker level. The "treatment pre-period mean Y" is the mean of the corresponding outcome prior to the ordinance for exposed workers, while the "treatment post-period mean Y" reports means after the ordinance for exposed workers. * $p < .05$, ** $p < .01$, *** $p < .001$.

Table C.4: Robustness – Effects on Delivery Earnings, Alternative Imputation Methods

	Base earnings		Tip earnings		Bonus+incentive+adjustment earnings		Total earnings	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
A. More-attached workers								
Treated × Post	155.311***	156.893***	-145.057***	-123.099***	16.713**	15.783**	28.569	51.115
	(35.658)	(33.994)	(27.722)	(22.190)	(5.243)	(4.945)	(56.697)	(51.480)
N worker-months	14,135	14,135	14,135	14,135	14,135	14,135	14,135	14,135
N workers	1,285	1,285	1,285	1,285	1,285	1,285	1,285	1,285
Treatment pre-pd mean Y	506.110	506.110	537.649	537.649	30.495	30.495	1069.992	1069.992
Treatment post-pd mean Y	548.647	548.647	225.993	225.993	43.836	43.836	815.776	815.776
B. Less-attached workers								
Treated × Post	33.223**	32.619**	-14.383**	-15.357*	3.643***	3.638**	22.755	21.227
	(10.788)	(11.881)	(5.302)	(5.972)	(1.031)	(1.234)	(15.788)	(17.519)
N worker-months	14,168	14,168	14,168	14,168	14,168	14,168	14,168	14,168
N workers	1,288	1,288	1,288	1,288	1,288	1,288	1,288	1,288
Treatment pre-pd mean Y	30.911	30.911	30.676	30.676	2.099	2.099	63.269	63.269
Treatment post-pd mean Y	71.701	71.701	24.784	24.784	4.603	4.603	100.980	100.980
Controls		✓		✓		✓		✓

Notes: This table presents estimates of β from Equation (1) replacing the indicators for event months interacted with the term Treat_i with a single indicator for the post-ordinance period. The outcomes are monthly delivery base earnings (columns 1-2), monthly delivery tip earnings (columns 3-4), monthly delivery bonus, incentive, and adjustment earnings (columns 5-6), and monthly delivery total earnings (columns 7-8), where total earnings include base earnings, tip earnings, bonus earnings, incentive earnings, and adjustment earnings, using alternative imputation methods. Tip earnings are imputed by subtracting base earnings and bonus earnings from the recorded total earnings in the raw data after imputing missing bonus earnings values with zeros. Bonus earnings are imputed by subtracting base earnings and tip earnings from the recorded total earnings in the raw data after imputing missing tip earnings values with zeros. Total earnings documented in this table are imputed as the sum of base earnings, tip earnings, bonus earnings, incentive earnings, and adjustment earnings after imputing missing tip and bonus earnings values with zeros. For a detailed description of the imputation procedure, see Appendix Section A.2. All outcomes are calculated in levels, inclusive of zero values. Worker fixed effects and event year-month fixed effects are included in the estimation. Columns 1, 3, 5, and 7 present estimates without including additional controls, and columns 2, 4, 6, and 8 present estimates including the full set of additional controls—a set of pre-policy individual worker covariates interacted with the indicator for the post-ordinance period. Covariates consist of delivery (rideshare) tasks, months active in delivery (rideshare) work, share of earnings from rideshare versus delivery work, and log total earnings, all measured during the pre-policy period. Panels (A) and (B) present estimates for the samples of more-attached and less-attached workers, respectively, defined as workers who performed delivery tasks above and below the median in the pre-policy period. Standard errors in parentheses are clustered at the worker level. The "treatment pre-period mean Y" is the mean of the corresponding outcome prior to the ordinance for exposed workers, while the "treatment post-period mean Y" reports means after the ordinance for exposed workers. * $p < .05$, ** $p < .01$, *** $p < .001$.

Table C.5: Robustness – Effects on Delivery Hours, Alternative Imputation Methods

	Total hours, with 1.25× imputation				Total hours, with 0.75× imputation			
	Levels		Logs		Levels		Logs	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
A. More-attached workers								
Treated × Post	-0.642	0.465	-0.056	-0.057	-1.261	-0.124	-0.080	-0.082
	(2.714)	(2.459)	(0.064)	(0.063)	(2.697)	(2.440)	(0.066)	(0.066)
N worker-months	14,135	14,135	9,905	9,905	14,135	14,135	9,905	9,905
N workers	1,285	1,285	1,265	1,265	1,285	1,285	1,265	1,265
Treatment pre-pd mean Y	54.631	54.631	3.718	3.718	54.128	54.128	3.700	3.700
Treatment post-pd mean Y	38.627	38.627	3.447	3.447	37.743	37.743	3.389	3.389
B. Less-attached workers								
Treated × Post	0.946	0.849	0.237	0.348**	0.783	0.715	0.233	0.344*
	(0.804)	(0.885)	(0.131)	(0.131)	(0.782)	(0.866)	(0.139)	(0.138)
N worker-months	14,168	14,168	4,160	4,160	14,168	14,168	4,160	4,160
N workers	1,288	1,288	941	941	1,288	1,288	941	941
Treatment pre-pd mean Y	3.413	3.413	1.563	1.563	3.259	3.259	1.423	1.423
Treatment post-pd mean Y	5.176	5.176	2.179	2.179	4.884	4.884	2.037	2.037
Controls		✓		✓		✓		✓

Notes: This table presents estimates of β from Equation (1) replacing the indicators for event months interacted with the term Treat_i with a single indicator for the post-ordinance period. The outcome is imputed monthly total hours on all delivery platforms using two alternative imputation methods: multiplying the imputed hours for non-DoorDash shifts that contain only one trip by 1.25 (columns 1-2 for levels and columns 3-4 for logs) and by 0.75 (columns 5-6 for levels and columns 7-8 for logs). For a detailed description of the imputation procedure, see Appendix Section A.4. Outcomes in levels are inclusive of zero values. Outcomes in logs corresponding to zero values in levels in a given event month are coded as missing in that month. Worker fixed effects and event year-month fixed effects are included in the estimation. Columns 1, 3, 5, and 7 present estimates without including additional controls, and columns 2, 4, 6, and 8 present estimates including the full set of additional controls—a set of pre-policy individual worker covariates interacted with the indicator for the post-ordinance period. Covariates consist of delivery (rideshare) tasks, months active in delivery (rideshare) work, share of earnings from rideshare versus delivery work, and log total earnings, all measured during the pre-policy period. Panels (A) and (B) present estimates for the samples of more-attached and less-attached workers, respectively, defined as workers who performed delivery tasks above and below the median in the pre-policy period. Standard errors in parentheses are clustered at the worker level. The "treatment pre-period mean Y" is the mean of the corresponding outcome prior to the ordinance for exposed workers, while the "treatment post-period mean Y" reports means after the ordinance for exposed workers. * $p < .05$, ** $p < .01$, *** $p < .001$.

Table C.6: Robustness – Effects on Delivery Pay Per Task, Continuous Exposure Effects

	Base pay per task		Tip pay per task		Bonus+incentive+adjustment pay per task		Total pay per task	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
A. More-attached workers								
Exposure × Post	4.115***	4.070***	-1.552***	-1.536***	0.804***	0.783***	3.423***	3.374***
	(0.211)	(0.211)	(0.140)	(0.136)	(0.147)	(0.147)	(0.277)	(0.279)
N worker-months	10,311	10,311	10,311	10,311	10,311	10,311	10,311	10,311
N workers	1,324	1,324	1,324	1,324	1,324	1,324	1,324	1,324
B. Less-attached workers								
Exposure × Post	4.238***	4.506***	-1.559***	-1.525***	1.186*	1.419*	3.895***	4.419***
	(0.449)	(0.466)	(0.277)	(0.287)	(0.562)	(0.602)	(0.750)	(0.786)
N worker-months	4,448	4,448	4,448	4,448	4,448	4,448	4,448	4,448
N workers	991	991	991	991	991	991	991	991
Controls		✓		✓		✓		✓

Notes: This table presents estimates of β from Equation (1) replacing the indicators for event months interacted with the term Treat_i with a single indicator for the post-ordinance period and replacing the binary exposure measure Treat_i with a continuous exposure measure as discussed in Section 3.2. The outcomes are delivery base pay per task (columns 1-2), delivery tip pay per task (columns 3-4), delivery bonus, incentive, and adjustment pay per task (columns 5-6), and delivery total pay per task (columns 7-8), where total pay includes base pay, tip pay, bonus pay, incentive pay, and adjustment pay. All outcomes are calculated in levels. Outcomes corresponding to zero delivery tasks in a given event month are coded as missing in that month. Worker fixed effects and event year-month fixed effects are included in the estimation. Columns 1, 3, 5, and 7 present estimates without including additional controls, and columns 2, 4, 6, and 8 present estimates including the full set of additional controls—a set of pre-policy individual worker covariates interacted with the indicator for the post-ordinance period. Covariates consist of delivery (rideshare) tasks, months active in delivery (rideshare) work, share of earnings from rideshare versus delivery work, and log total earnings, all measured during the pre-policy period. Panels (A) and (B) present estimates for the samples of more-attached and less-attached workers, respectively, defined as workers who performed delivery tasks above and below the median in the pre-policy period. Standard errors in parentheses are clustered at the worker level. * $p < .05$, ** $p < .01$, *** $p < .001$.

Table C.7: Robustness – Effects on Delivery Tasks, Continuous Exposure Effects

	Any		Levels		Logs	
	(1)	(2)	(3)	(4)	(5)	(6)
A. More-attached workers						
Exposure × Post	0.004	-0.015	-16.588**	-13.123**	-0.266***	-0.266***
	(0.024)	(0.023)	(5.270)	(4.461)	(0.066)	(0.066)
N worker-months	14,784	14,784	14,784	14,784	10,311	10,311
N workers	1,344	1,344	1,344	1,344	1,324	1,324
B. Less-attached workers						
Exposure × Post	0.006	-0.022	-0.151	-0.461	0.030	0.146
	(0.023)	(0.021)	(1.394)	(1.515)	(0.133)	(0.130)
N worker-months	14,883	14,883	14,883	14,883	4,448	4,448
N workers	1,353	1,353	1,353	1,353	991	991
Controls		✓		✓		✓

Notes: This table presents estimates of β from Equation (1) replacing the indicators for event months interacted with the term Treat_i with a single indicator for the post-ordinance period and replacing the binary exposure measure Treat_i with a continuous exposure measure as discussed in Section 3.2. The outcomes are an indicator for performing any delivery tasks per month (columns 1-2), the number of monthly completed delivery tasks in levels (columns 3-4), and the number of monthly completed delivery tasks in logs (columns 5-6). Outcomes in levels are inclusive of zero values. Outcomes in logs corresponding to zero values in levels in a given event month are coded as missing in that month. Worker fixed effects and event year-month fixed effects are included in the estimation. Columns 1, 3, and 5 present estimates without including additional controls, and columns 2, 4, and 6 present estimates including the full set of additional controls—a set of pre-policy individual worker covariates interacted with the indicator for the post-ordinance period. Covariates consist of delivery (rideshare) tasks, months active in delivery (rideshare) work, share of earnings from rideshare versus delivery work, and log total earnings, all measured during the pre-policy period. Panels (A) and (B) present estimates for the samples of more-attached and less-attached workers, respectively, defined as workers who performed delivery tasks above and below the median in the pre-policy period. Standard errors in parentheses are clustered at the worker level. * $p < .05$, ** $p < .01$, *** $p < .001$.

Table C.8: Robustness – Effects on Delivery Earnings, Continuous Exposure Effects

	Base earnings		Tip earnings		Bonus+incentive+adjustment earnings		Total earnings	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
A. More-attached workers								
Exposure × Post	157.151***	160.248***	-146.780***	-124.352***	17.516***	16.594***	26.870	51.517
	(36.426)	(34.746)	(28.066)	(22.764)	(5.235)	(4.972)	(58.146)	(52.955)
N worker-months	14,784	14,784	14,784	14,784	14,784	14,784	14,784	14,784
N workers	1,344	1,344	1,344	1,344	1,344	1,344	1,344	1,344
B. Less-attached workers								
Exposure × Post	35.022**	34.101**	-13.365*	-13.977*	4.598***	4.651***	26.369	24.863
	(11.425)	(12.301)	(5.596)	(6.251)	(1.145)	(1.321)	(16.694)	(18.160)
N worker-months	14,883	14,883	14,883	14,883	14,883	14,883	14,883	14,883
N workers	1,353	1,353	1,353	1,353	1,353	1,353	1,353	1,353
Controls		✓		✓		✓		✓

Notes: This table presents estimates of β from Equation (1) replacing the indicators for event months interacted with the term Treat_i with a single indicator for the post-ordinance period and replacing the binary exposure measure Treat_i with a continuous exposure measure as discussed in Section 3.2. The outcomes are monthly delivery base earnings (columns 1-2), monthly delivery tip earnings (columns 3-4), monthly delivery bonus, incentive, and adjustment earnings (columns 5-6), and monthly delivery total earnings (columns 7-8), where total earnings include base earnings, tip earnings, bonus earnings, incentive earnings, and adjustment earnings. All outcomes are calculated in levels, inclusive of zero values. Worker fixed effects and event year-month fixed effects are included in the estimation. Columns 1, 3, 5, and 7 present estimates without including additional controls, and columns 2, 4, 6, and 8 present estimates including the full set of additional controls—a set of pre-policy individual worker covariates interacted with the indicator for the post-ordinance period. Covariates consist of delivery (rideshare) tasks, months active in delivery (rideshare) work, share of earnings from rideshare versus delivery work, and log total earnings, all measured during the pre-policy period. Panels (A) and (B) present estimates for the samples of more-attached and less-attached workers, respectively, defined as workers who performed delivery tasks above and below the median in the pre-policy period. Standard errors in parentheses are clustered at the worker level. * $p < .05$, ** $p < .01$, *** $p < .001$.

Table C.9: Robustness – Effects on Delivery Hours and Active Delivery Days, Continuous Exposure Effects

	Total hours, with imputation				DoorDash hours				Total active days			
	Levels		Logs		Levels		Logs		Levels		Logs	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
A. More-attached workers												
Exposure × Post	-1.197	0.016	-0.061	-0.066	-0.547	0.297	0.044	0.037	0.289	0.231	-0.010	-0.017
	(2.798)	(2.541)	(0.066)	(0.066)	(2.344)	(2.196)	(0.086)	(0.085)	(0.455)	(0.427)	(0.044)	(0.044)
N worker-months	13,959	13,959	9,795	9,795	14,784	14,784	6,587	6,587	14,784	14,784	10,311	10,311
N workers	1,269	1,269	1,249	1,249	1,344	1,344	913	913	1,344	1,344	1,324	1,324
B. Less-attached workers												
Exposure × Post	1.093	0.976	0.195	0.317*	0.491	0.600	0.055	0.133	0.341	0.273	0.103	0.189*
	(0.838)	(0.911)	(0.136)	(0.136)	(0.567)	(0.645)	(0.208)	(0.198)	(0.241)	(0.249)	(0.095)	(0.095)
N worker-months	14,344	14,344	4,270	4,270	14,883	14,883	1,576	1,576	14,883	14,883	4,448	4,448
N workers	1,304	1,304	957	957	1,353	1,353	389	389	1,353	1,353	991	991
Controls		✓		✓		✓		✓		✓		✓

Notes: This table presents estimates of β from Equation (1) replacing the indicators for event months interacted with the term Treat_i with a single indicator for the post-ordinance period and replacing the binary exposure measure Treat_i with a continuous exposure measure as discussed in Section 3.2. The outcomes are imputed monthly total hours on all delivery platforms (columns 1-2 for levels and columns 3-4 for logs), monthly total hours on DoorDash (columns 5-6 for levels and columns 7-8 for logs), and monthly active days on all delivery platforms (columns 9-10 for levels and columns 11-12 for logs). Outcomes in levels are inclusive of zero values. Outcomes in logs corresponding to zero values in levels in a given event month are coded as missing in that month. Worker fixed effects and event year-month fixed effects are included in the estimation. Columns 1, 3, 5, 7, 9, and 11 present estimates without including additional controls, and columns 2, 4, 6, 8, 10, and 12 present estimates including the full set of additional controls—a set of pre-policy individual worker covariates interacted with the indicator for the post-ordinance period. Covariates consist of delivery (rideshare) tasks, months active in delivery (rideshare) work, share of earnings from rideshare versus delivery work, and log total earnings, all measured during the pre-policy period. Panels (A) and (B) present estimates for the samples of more-attached and less-attached workers, respectively, defined as workers who performed delivery tasks above and below the median in the pre-policy period. Standard errors in parentheses are clustered at the worker level. * $p < .05$, ** $p < .01$, *** $p < .001$.

Table C.10: Robustness – Effects on Delivery Time Use, Continuous Exposure Effects

	Tasks per hour				Utilization rate		Task wait time (min)		Distance between tasks (mi)	
	All apps, with imputation		DoorDash only							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
A. More-attached workers										
Exposure × Post	-0.352***	-0.346***	-0.346***	-0.347***	-0.112***	-0.110***	5.785***	5.713***	0.294***	0.314***
	(0.027)	(0.027)	(0.037)	(0.037)	(0.023)	(0.022)	(0.614)	(0.636)	(0.071)	(0.073)
N worker-months	9,795	9,795	6,587	6,587	3,953	3,953	4,707	4,707	4,625	4,625
N workers	1,249	1,249	913	913	632	632	698	698	686	686
B. Less-attached workers										
Exposure × Post	-0.274***	-0.325***	-0.422***	-0.412***	-0.206***	-0.217***	6.014***	5.091***	0.197	0.127
	(0.071)	(0.069)	(0.094)	(0.089)	(0.062)	(0.065)	(1.335)	(1.420)	(0.195)	(0.231)
N worker-months	4,270	4,270	1,576	1,576	1,574	1,574	1,680	1,680	1,647	1,647
N workers	957	957	389	389	393	393	416	416	407	407
Controls		✓		✓		✓		✓		✓

Notes: This table presents estimates of β from Equation (1) replacing the indicators for event months interacted with the term Treat_i with a single indicator for the post-ordinance period and replacing the binary exposure measure Treat_i with a continuous exposure measure as discussed in Section 3.2. The outcomes are the monthly average number of delivery tasks completed per hour (columns 1-2 for all delivery platforms using imputed monthly total hours, and columns 3-4 for DoorDash which reports hours directly), the monthly utilization rate (columns 5-6), monthly average wait time (minutes) per task (columns 7-8), and monthly average miles traveled between tasks (columns 9-10). Utilization rates, task wait times, and distances between tasks are calculated for Uber Eats and Grubhub, given data availability. All outcomes are calculated in levels. Outcomes corresponding to zero tasks completed on the relevant delivery platform(s) in a given event month are coded as missing in that month. Worker fixed effects and event year-month fixed effects are included in the estimation. Columns 1, 3, 5, 7, and 9 present estimates without including additional controls, and columns 2, 4, 6, 8, and 10 present estimates including the full set of additional controls—a set of pre-policy individual worker covariates interacted with the indicator for the post-ordinance period. Covariates consist of delivery (rideshare) tasks, months active in delivery (rideshare) work, share of earnings from rideshare versus delivery work, and log total earnings, all measured during the pre-policy period. Panels (A) and (B) present estimates for the samples of more-attached and less-attached workers, respectively, defined as workers who performed delivery tasks above and below the median in the pre-policy period. Standard errors in parentheses are clustered at the worker level. * $p < .05$, ** $p < .01$, *** $p < .001$.

D Model

In this appendix, we study the effects of a per-task minimum pay regulation in a simple model of the app-based delivery market, which for brevity we refer to as the "gig delivery" or "platform delivery" market. We closely follow the model of the ride-sharing market introduced by Hall, Horton and Knoepfle (2023) and extended by Fisher (2024*a*), deriving similar results in a somewhat simplified setting.

We assume throughout that the gig delivery market is small compared to the rest of the labor market and find that the minimum pay regulation can leave unchanged, increase, or decrease drivers' earnings depending on whether or not there is free entry and depending on the elasticity of demand for delivery services. If the gig delivery market faces free entry of drivers, a binding minimum pay regulation will have no effect on drivers' per period earnings, as the equilibrium value of doing delivery work must always remain equal to the fixed, constant outside option available to all workers (Hall, Horton and Knoepfle, 2023). Without free entry, the effect of a binding minimum pay regulation on drivers' earnings depends on the elasticity of demand for delivery services, consistent with the results in Fisher (2024*a*). Drivers' per-period earnings decrease when this demand curve is elastic, as the decline in trips per period exceeds the increase in pay per trip. Inversely, per-period earnings increase when demand is inelastic, as trips decline less than the increased in pay per trip.

D.1 Labor supply to the delivery market

Consider a market for gig delivery drivers where a single platform sets a per-delivery pay rate equal to w.[46] All workers, indexed by i, inelastically supply one period of full-time labor and can freely choose whether to spend the period doing platform delivery work or pursuing an outside option, which idiosyncratically varies across workers. This outside option includes both the value of work outside the gig delivery market and the value of non-work time.

If a worker chooses to drive in the gig delivery sector, their per-period earnings, $\omega^g = \theta w$, depend both on the platform-determined pay rate w for time spent completing delivery tasks and the share θ of the period spent engaged in those tasks. Worker j values the per-period outside option at ω_j^0 and chooses to do delivery work if $\omega^g > \omega_j^0$. If the value of the outside option is distributed according to some distribution $F(\cdot)$, the share of workers doing delivery work is $F(\omega^g)$. Assuming outside options follow a logistic distribution with scale parameter $\rho = 1/\sigma$ (where high ρ and low σ indicate greater

[46] The single platform assumption is not crucial here, so long as the minimum pay standard is binding in what follows.

variance in outside options), the share of workers doing delivery work is

$$\pi^g = Pr[\omega_j^0 < \omega^g] = \frac{e^{\sigma \ln \omega^g}}{1 + e^{\sigma \ln \omega^g}}. \tag{3}$$

The elasticity of this choice probability with respect to gig delivery earnings (given a fixed distribution of outside options) is

$$\frac{\partial \ln \pi^g}{\partial \ln \omega^g} = \sigma - \frac{1}{1 + e^{\sigma \ln \omega^g}} \cdot \frac{\partial}{\partial \ln \omega^g}(1 + e^{\sigma \ln \omega^g}). \tag{4}$$

Focusing on the last term,

$$\frac{\partial}{\partial \ln \omega^g}(1 + e^{\sigma \omega^g}) = \sigma e^{\sigma \ln \omega^g}$$

Plugging this back in to (4) yields

$$\frac{\partial \ln \pi^g}{\partial \ln \omega^g} = \sigma(1 - \pi^g). \tag{5}$$

Letting $\overline{L}$ be the total amount of labor across both markets, the quantity of labor supplied to the gig delivery market in each period is

$$S(\omega^g) = \overline{L}\pi^g,$$

and the associated labor supply elasticity is

$$\varepsilon^S \equiv \frac{\partial \ln S(\omega^g)}{\partial \ln \omega^g} = \frac{\partial \ln \pi^g}{\partial \ln \omega^g} = \sigma(1 - \pi^g).$$

We assume that the delivery market is small in comparison to the overall labor market, such that $\pi^g \to 0$ and the labor supply elasticity facing the market is the constant σ. This is intuitive – as workers weigh wages more strongly than their idiosyncratic preferences, their labor supply will be more responsive to delivery market wages. The small market assumption also implies that changes in the gig delivery market have no effect on earnings in the outside market, ω^0, making the partial derivative in (4) applicable even in equilibrium.

D.2 Labor demand under a minimum pay standard

In contrast to the ride-hailing markets analyzed in Hall, Horton and Knoepfle (2023) and Fisher (2024*a*), the payments made to drivers in the gig delivery market are not directly paid by consumers.[47] Rather, consumers are presented with a cost of delivered items that reflects the price of food or merchandise charged by the restaurant or retailer and

[47]Recently, some ride-hailing platforms appear to have changed their driver payment practices to match gig delivery more closely, but this change emerged after Hall, Horton and Knoepfle (2023) and Fisher (2024*a*) were written.

fees collected by the platform that are used both to compensate drivers and generate platform revenues. For the purposes of our analysis, it is not necessary to fully derive the determinants of demand for delivery drivers, which will depend jointly on consumer, restaurant, and platform behavior. Nor is it necessary to provide a full account for the objective functions that guide the platform (or the set of competing platforms) in their choice of w (see Fisher (2024*a*) for such a model). Instead we simply assume:

1. That there is *some* no-intervention equilibrium pay rate w^e chosen by the platform to maximize its profits, that is below the minimum rate $\bar{w}$ set by policy, and
2. For $\bar{w} > w^e$, the total demand for deliveries in the market is given by the downward-sloping demand curve $D(w)$, which is a reduced-form object reflecting the pass-through of costs to consumers and restaurants and any resulting decline in quantity demanded for deliveries completed by gig drivers.

D.3 Market equilibrium

Importantly, the per-trip pay rate (which is set by the firm or by a binding minimum policy rate) does not in itself determine worker earnings ω^g, which also depend on the endogenous fraction of time the worker spends doing deliveries during a period. For simplicity, we assume the platform distributes jobs evenly across workers, so that the share of the period spent completing delivery tasks is

$$\theta = \frac{D(w)}{S(\omega^g)} = \frac{D(w)}{S(w \cdot \theta)}$$

Re-arranging yields the equilibrium market-clearing condition, which is a simplified version of the corresponding conditions in Hall, Horton and Knoepfle (2023) (their equation (1)) and Fisher (2024*a*) (their equation (2)).

$$D(w) = \theta \cdot S(w \cdot \theta) \tag{6}$$

Since this condition does not equate supply and demand for delivery tasks, the pay rate w is not pinned down by this equilibrium condition, which depends upon both w and θ. Instead, the pay rate is determined by platform optimization and competition or by a binding minimum pay rate. Equation (6) should be understood as a constraint that specifies what the equilibrium value of θ will be for any specified choice of w. In particular, equation (6) implicitly defines the equilibrium market tightness (i.e. share of the period spent completing paying trips) as a function of the wage, $\theta(w)$. Totally differentiating equation (6), we can solve for $\theta'(w)$.

$$D'(w) = \theta'(w) \cdot S(w \cdot \theta(w)) + \theta(w) \cdot S'(w \cdot \theta(w))(\theta(w) + w \cdot \theta'(w))$$

Dividing through by $D(w)/w$ and noting $D(w)/w = \theta(w)S(w\theta(w))/w$,

$$\frac{D'(w)}{D(w)/w} = \frac{\theta'(w)}{\theta(w)/w} + \frac{S'(w\theta(w))w\theta'(w)}{S(w\theta(w))/w} + \frac{S'(w\theta(w))}{S(w\theta(w))/w\theta(w)}.$$

Multiplying the numerator and denominator of the middle term by $\theta(w)$,

$$\frac{D'(w)}{D(w)/w} = \frac{\theta'(w)}{\theta(w)/w} + \frac{S'(w\theta(w))}{S(w\theta(w))/w\theta(w)} \cdot \frac{\theta'(w)}{\theta(w)/w} + \frac{S'(w\theta(w))}{S(w\theta(w))/w\theta(w)}.$$

Defining elasticities as $\varepsilon^H = \frac{H'(x)}{H(x)/x}$,

$$\varepsilon^D = \varepsilon^\theta + \varepsilon^S \cdot \varepsilon^\theta + \varepsilon^S$$

$$\varepsilon^\theta = \frac{\varepsilon^D - \varepsilon^S}{1 + \varepsilon^S} = \frac{\varepsilon^D - \sigma}{1 + \sigma}$$

where $\varepsilon^D < 0$ is the elasticity of trip demand with respect to the per-trip pay rate, $\varepsilon^S = \sigma > 0$ is the elasticity of labor supply to per period earnings, and $\varepsilon^\theta < 0$ is the elasticity of θ to the per-trip pay rate. Notably, if both the elasticities of demand and supply are constant, then ε^θ is constant.

D.4 The effects of a minimum pay standard on per-period earnings

Given that earnings per period are $\omega^g = \theta w$, the elasticity of earnings to the binding per-trip minimum pay rate is

$$\varepsilon^\omega = 1 + \varepsilon^\theta.$$

Importantly, since $\varepsilon^\theta < 0$, this elasticity can be negative—that is, a higher per-trip minimum pay standard can lead to lower daily earnings if trips per period decline sufficiently. More specifically, the model with free entry implies the following key result:

Increasing the minimum pay standard increases driver daily pay if and only if

$$\varepsilon^\theta = \frac{\varepsilon^D - \sigma}{1 + \sigma} > -1$$

and has no effect if

$$\varepsilon^\theta = \frac{\varepsilon^D - \sigma}{1 + \sigma} = -1.$$

A few special cases are worth highlighting:

1. If worker mobility is perfect (no dispersion in outside options) such that $\sigma = \infty$, then $\varepsilon^\theta = -1$ and minimum pay standards have no impact on driver earnings. Intuitively, with free entry and no dispersion in outside options, the equilibrium value of doing delivery work must always remain equal to the fixed, constant outside option available to all workers. Any increase in $\bar{w}$ will lead to an increase in drivers that causes a decline in θ exactly offsetting the impact on pay per trip.

2. With a finite supply elasticity, higher minimum pay standards lead to lower driver earnings whenever the elasticity of demand is less than -1. Intuitively, when demand is elastic, the decline in trips per period exceeds the increase in pay per trip. This

3. Similarly, with a finite supply elasticity, higher minimum pay standards lead to higher daily driver earnings whenever the elasticity of demand is greater than -1. Intuitively, when demand is inelastic, the decline in trips per period is smaller than the increase in pay per trip.

In summary, a trip-level minimum pay standard will only increase driver earnings in settings where there is not free entry of drivers ($\sigma <$) and where the market-level elasticity of demand for drivers is sufficiently small ($|\varepsilon^D| < 1$). The first condition highlights a key tension in regulating gig work: increased pay necessitates the imposition of barriers to entry, which will tend to undermine the flexibility that the gig economy offers to workers. The second condition is more general—it is similarly the case in the traditional analysis of minimum wages in competitive labor markets that higher minimum wages will lower aggregate payroll if the absolute elasticity of demand is greater than one. However, in the traditional setting, there is a well-defined group of job holders who benefit from higher wages even when demand is highly elastic, with any reduction in demand manifesting as reduced employment levels. By contrast, in the platform gig work setting where tasks are allocated across all job seekers, every *individual* experiences a reduction in expected pay.

D.5 Discussion

Our empirical analysis of the Seattle minimum pay regulation finds a lack of earnings effect. In the month following the regulation entering into force, incumbent drivers saw an increase in monthly earnings, but those earnings subsequently fell back to their pre-treatment level, due to a reduction in the tasks available to incumbent drivers (Figures 7 and 8). We find no evidence that hours participating in gig delivery work fell substantially, consistent with earnings returning to baseline due to drivers struggling to find delivery tasks. In the context of the model, the lack of earnings effect implies that $\varepsilon^\theta = -1$, which could be driven either by free entry or unit-elastic demand. However, the descriptive evidence in Figure 5 suggests that free entry drives our findings, since the post-regulation share of tasks done by new entrants grows more in Seattle than in the comparison region.

A few important assumptions of the model are worth emphasizing. The model does not incorporate the pecuniary or effort costs of driving, and we implicitly assume that workers do not derive utility from idle time waiting for new tasks. These two assumptions imply that a larger value of θ is unambiguously negative for drivers – it implies lower earnings for a given per-task pay rate without any offsetting benefit. These assumptions align with our empirical finding that drivers face longer wait times and distances between trips under the minimum pay law (Table 6). Rather than saving on transportation costs or enjoying leisure time while waiting for new tasks, drivers appear to spend more time driving back to high-demand locations to obtain new tasks after the regulation went into effect.

Overall, in the context of the model, our findings suggest the the gig delivery market is subject to approximately free entry of drivers. This market feature implies that if policymakers seek to improve gig delivery drivers' earnings, they will struggle to do so without imposing some form of entry barrier, such as those traditionally governing entry to non-platform taxi markets in many cities. However, even with entry barriers, the qualitative effect of a per-task minimum pay regulation will depend on the elasticity of demand for delivery services; if that demand is elastic, the minimum pay regulation will still reduce drivers' earnings.